

# American Sociological Review

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## Four Papers on Stratification

**Szonja Szelenyi**

Social Inequality and Party Membership

**Amy S. Wharton and James N. Baron**

Impact of Gender Segregation

**Mark Abrahamson and Lee Sigelman**

Occupational Sex Segregation

**Kazimierz M. Slomczynski and Tadeusz K. Krauze**

Similarity in Social Mobility Patterns

**John R. Sutton**

Doing Time

## Four Papers on Educational Matters

**Von Bakanic, Clark McPhail, and Rita J. Simon**

Manuscript Review and Decision-Making Process

**Paul D. Allison and J. Scott Long**

Mobility of Scientists

**Maureen T. Hallinan and Richard A. Williams**

Students' Interracial Friendships

**Karl L. Alexander, Doris R. Entwistle, and**

**Maxine S. Thompson**

School Performance and Status Relations

**Cecilia L. Ridgeway**

Nonverbal Behavior

## Research Notes

**Frank F. Furstenberg, Jr., S. Phillip Morgan, and**

**Paul D. Allison**

Paternal Participation

**Darrell Steffensmeier, Cathy Streifel, and Miles D. Harer**

Relative Cohort Size and Youth Crime

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P4417

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Am 35

# AMERICAN SOCIOLOGICAL REVIEW

October 1987

Vol. 52, No. 5

Contributors to this issue ..... iv

Editor's comment ..... vi

## ARTICLES

Social Inequality and Party Membership: Patterns of Recruitment into the Hungarian Socialist Workers' Party ..... *Szonja Szélényi* 559

So Happy Together? The Impact of Gender Segregation on Men at Work ..... *Amy S. Wharton and James N. Baron* 574

Occupational Sex Segregation in Metropolitan Areas ... *Mark Abrahamson and Lee Sigelman* 588

Cross-National Similarity in Social Mobility Patterns: A Direct Test of the Featherman-Jones-Hauser Hypothesis ..... *Kazimierz M. Slomczynski and Tadeusz K. Krauze* 598

Doing Time: Dynamics of Imprisonment in the Reformist State ..... *John R. Sutton* 612

The Manuscript Review and Decision-Making Process ..... *Von Bakanic, Clark McPhail, and Rita J. Simon* 631

Interuniversity Mobility of Academic Scientists ..... *Paul D. Allison and J. Scott Long* 643

The Stability of Students' Interracial Friendships ..... *Maureen T. Hallinan and Richard A. Williams* 653

School Performance, Status Relations, and the Structure of Sentiment: Bringing the Teacher Back In ..... *Karl L. Alexander, Doris R. Entwisle, and Maxine S. Thompson* 665

Nonverbal Behavior, Dominance, and the Basis of Status in Task Groups ..... *Cecilia L. Ridgeway* 683

## RESEARCH NOTES

Paternal Participation and Children's Well-Being After Marital Dissolution ..... *Frank F. Furstenberg, Jr., S. Philip Morgan, and Paul D. Allison* 695

Relative Cohort Size and Youth Crime in the United States, 1953-1984 ..... *Darrell Steffensmeier, Cathy Streifel, and Miles D. Harer* 702

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ASA Journals ..... 711

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(Revised January 1987)

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Submission of manuscripts to a professional journal clearly implies commitment to publish in that journal. The competition for journal space requires a great deal of time and effort on the part of editorial readers whose main compensation for this service is the opportunity to read papers prior to publication and the gratification associated with discharge of professional obligations. For these reasons the ASA regards submission of a manuscript to a professional journal while that paper is under review by another journal as unacceptable.

Section II.B4, ASA Code of Ethics

**AUTHOR'S RESPONSIBILITY:** *Manuscripts which do not adhere exactly to these suggestions will be immediately returned for revision. Only articles in the form specified below will be sent out for refereeing.*

The third edition of the *Publication Manual of the American Psychological Association* (available from A.P.A., P.O. Box 2710, Hyattsville, MD 20784) is very helpful in clarifying headings, abbreviations, table preparation, and the like.

Since manuscripts are evaluated as anonymous submissions, please keep identifying material out of the manuscript. Attach a cover page giving authorship, institutional affiliation, acknowledgments, and the date the article is submitted. Provide only the title as identification on the manuscript and abstract. Every effort should be made by the author to see that the manuscript itself contains no clues as to the author's identity.

## Submission and Preparation of Manuscripts

1. Submit four (4) copies and retain the original for your files. Copies may be Xerox, mimeograph, or multilith, but not carbons. Manuscripts must be accompanied by a \$10.00 manuscript submission fee, payable to the American Sociological Association.

2. Enclose a stamped self-addressed postcard for acknowledgment of manuscript receipt. Since manuscripts will not be returned by reviewers to the editorial office, the editorial office will not return them to authors.

3. All copy must be *typed, double-spaced* (including indented material, footnotes, and references) on 8½ by 11 inch white opaque paper. Line must not exceed six (6) inches. Margins must be a minimum of 1¼ inch on left and foot.

4. Include four (4) copies of an abstract of *no more* than 150 words.

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6. Illustrations submitted with the final draft must be of professional quality, ready for reproduction, executed on white paper or vellum, in black ink, with clear, medium weight, black lines and figures. All lettering on figures should be executed by an artist in pen and ink, by means of dry transfer letters, or by applying typeset material to the prepared artwork. Typewritten lettering should not appear in illustrations. Figures should be capable of legible reduction to a width size no larger than 5⅞ inch (full page) or 2⅞ inch (one column); they should be numbered consecutively, and the number and author's name should be penciled lightly on the back of each. All illustrations must have captions, which should not appear on the artwork but should be typed, double-spaced, on a sheet at the end of the manuscript.

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8. Three kinds of footnotes are possible each serving a different purpose:

*a. Content footnotes.* Content footnotes are explanations or amplifications of the text. Because they are distracting to readers and *expensive* to include in printed material, an author should include important information in the text and omit irrelevant information. *Content footnotes will not be allowed generally.*

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Text footnotes should be numbered consecutively throughout the article with superscript arabic



numerals. If after a footnote occurs it is later mentioned, use a parenthetical note "(see note 3)," rather than the superscript number.

*b. Reference footnotes.* Footnotes are used for reference purposes only to cite material of limited availability. Acceptable reference footnotes include (i) legal citations, which should follow the footnote style of *A Uniform System of Citation* (1967) published by the Harvard Law Review Association, (ii) copyright permission footnotes, (iii) unpublished works and works in progress.

*c. Table footnotes.* Table footnotes are appended *only* to a specific table. Footnotes to a table should be lettered consecutively *within* each table with superscript lowercase letters. (See 5.)

9. Acknowledgments, credits, and grant numbers are placed on the title page with an asterisk.

## Reference Format

*A. In the text:* All source references are to be identified at the appropriate point in the text by the last name of the author, year of publication, and pagination where needed. Identify subsequent citations of the same source in the same way as the first. Examples follow:

1. If author's name is in the text, follow it with year in parentheses ["... Duncan (1959) ..."].
2. If author's name is not in the text, insert in parentheses the last name and year ["... (Gouldner 1963) ..."].
3. Pagination follows year of publication after a comma ["... Kuhn (1970, p. 71)."].
4. Give both last names for dual authors. Give all last names on first citation in text for more than two authors; thereafter use "et al." in the text. When two authors have the same last names, include initials in the text. For institutional authorship, supply minimum identification from the beginning of the complete citation ["... (U.S. Bureau of the Census 1963, p. 117) ..."].
5. Separate a series of references with semicolons and enclose them within a single pair of parentheses ["... (Burgess 1968; Marwell et al. 1971, pp. 386-87; Cohen 1962) ..."].

*B. In the appendix:* List all items alphabetically by author and, within author, by year of publication in an appendix titled "REFERENCES." The reference appendix must be complete and include all references in the text. The use of "et al." is not acceptable in the appendix; list the names of all authors using full first names. (See A.4. for text format.)

If there is more than one reference to the same author and year, distinguish them by the letters a, b, etc. added to the year ["... (Levy 1965a, p. 331) ..."].

The first letter of each word in an article title should be capitalized. Titles of books and journals are printed in italics, so each word of the title should be underlined.

Give the publisher's name in as brief a form as is fully intelligible. For example, John A. Wiley and Sons should be "Wiley."

If the cited material is unpublished, use "forthcoming" with name of journal or publisher; otherwise use "unpublished."

Examples follow:

1. *Books:*  
Mason, Karen O. 1974. *Women's Labor Force Participation and Fertility*. Research Triangle Park, NC: National Institutes of Health.  
U.S. Bureau of the Census. 1960. *Characteristics of Population*. Vol. 1. Washington, D.C.: U.S. Government Printing Office.
2. *Periodicals:*  
Conger, Rand D. Forthcoming. "The Effects of Positive Feedback on Direction and Amount of Verbalization in a Social Setting." *Pacific Sociological Review*.  
Goodman, Leo A. 1974a. "Exploratory Latent Structure Analysis Using Both Identifiable and Unidentifiable Models." *Biometrika* 61:215-31.  
———. 1974b. "The Analysis of Systems of Qualitative Variables When Some of the Variables Are Unobservable. Part I—A Modified Latent Structure Approach." *American Journal of Sociology* 79:1179-1259.
3. *Collections:*  
Clausen, John A. 1972. "The Life Course of Individuals." Pp. 457-514 in *Aging and Society*, vol. 3, *A Sociology of Age Stratification*, edited by M.W. Riley, M. Johnson, and A. Foner. New York: Russell Sage.  
Elder, Glen H. 1975. "Age Differentiation and the Life Course." Pp. 165-90 in *Annual Review of Sociology*, vol. 1, edited by A. Inkeles, J. Coleman, and N. Smelser. Palo Alto, CA: Annual Reviews.

See 1986 and later issues for further examples.



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■ MARK ABRAHAMSON (Occupational Sex Segregation in Metropolitan Areas) is Professor of Sociology, University of Connecticut, Storrs. His current research is examining the way the community context of formal organizations influences their form and their effectiveness. LEE SIGELMAN is Dean of the Faculty of Social and Behavioral Sciences and Professor of Political Science, University of Arizona. He has written on a broad range of topics in political science and sociology, focusing in recent years on issues related to public opinion and mass communication.

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■ JOHN R. SUTTON (Doing Time: Dynamics of Imprisonment in the Reformist State), Assistant Professor, Department of Sociology, Princeton University, is studying the expansion of a number of different social control institutions in the Progressive-era United States. His book *Stubborn Children: Controlling Delinquency in the United States, 1640-1981* is due to be published soon by the University of California Press.

■ VON BAKANIC (The Manuscript Review and Decision-Making Process) is Assistant Professor, Department of Sociology and Anthropology, University of Southern Mississippi. She is currently working on a content analysis of referees' comments in manuscript reviews. She is also gathering data from other journals for an interdisciplinary comparison of manuscript review and decision-making processes. CLARK MCPHAIL is Associate Professor, Department of Sociology, University of Illinois at Urbana-Champaign. His current theoretical and empirical work concerns alternating sequences of individual and collective behavior in large religious, sport, and political gatherings. RITA J. SIMON is Dean, School of Justice, The American University. Praeger has recently published her *Transracial Adoptees and Their Families*, with Howard Altstein, and will publish her forthcoming book, *The Insanity Defense on Trial*, with David E. Aaronson.

■ PAUL D. ALLISON (Interuniversity Mobility of Academic Scientists) is Professor of Sociology, University of Pennsylvania. He is continuing work with J. Scott Long on the effect of job mobility on scientists' research productivity. Other current research interests include the analysis of change scores, and methods for missing data. His *Event History Analysis* was published in 1984. J. SCOTT LONG is Associate Professor of Sociology, Washington State University. His current substantive research involves gender stratification science. He recently edited *Common Problems in Quantitative Social Research* forthcoming from Sage University Press. He was recently named editor of *Sociological Methods and Research*.

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■ KARL L. ALEXANDER (School Performance, Status Relations, and the Structure of Sentiment: Bringing the Teacher Back In) is Professor and Chair, Department of Sociology, Johns Hopkins University. The BSS has been his main research commitment for the last several years, although he maintains an active interest in curriculum effects at the secondary level and in patterns of college access at the postsecondary level. DORIS R. ENTWISLE, Professor of Sociology and Engineering Science at Johns Hopkins, is currently preoccupied with the sociology of human development. Karl Alexander and she are preparing a monograph dealing with how social structural factors impinge on children's achievement in the first two years of formal schooling. MAXINE S. THOMPSON, Associate Professor of Sociology, North Carolina State University, is currently doing work on household living arrangements and children's school behaviors. Her research focuses on the influence of personal resources, social supports, and coping patterns in different family settings to major life cycle transitions such as parenting and schooling.

■ CECILIA L. RIDGEWAY (Nonverbal Behavior, Dominance, and the Basis of Status in Task Groups) is Professor of Sociology, University of Iowa. She is interested in status processes and gender stratification and is currently studying the process of legitimation in informal status orders and the relationship of this to dominance behavior. Recent papers include "Expectations, Legitimation, and Dominance Behavior in Task Groups" (with Joseph Berger), *ASR*, October 1986, and "Gender Differences in Task Groups: A Status and Legitimacy Account," forthcoming in *Status Generalization: New Theory and Research*, edited by Murray Webster, Jr., and Martha Foschi, Stanford University Press. She is also the author of *The Dynamics of Small Groups*, St. Martin's Press, 1983.

■ FRANK F. FURSTENBERG, JR. (Paternal Participation and Children's Well-Being After

Marital Dissolution) is Professor of Sociology at the University of Pennsylvania and a Research Associate in the Population Studies Center. His recent books include *Recycling the Family* with Graham Spanier (1984), *The New American Grandparent* with Andrew Cherlin, and *Adolescent Mothers in Later Life* with J. Brooks-Gunn and S. Philip Morgan. He is conducting a longitudinal study with Morgan and Brooks-Gunn of poverty across generations. S. PHILIP MORGAN is Assistant Professor of Sociology, Department of Sociology and Population Studies Center, University of Pennsylvania. Forthcoming books include *First Births in America: Changes in the Timing of Parenthood* (with R. R. Rindfuss and C. G. Swicegood, University of California Press, 1988) and *Adolescent Mothers in Later Life* (with Frank F. Furstenberg, Jr. and J. Brooks Gunn, Cambridge University Press, 1987). PAUL D. ALLISON is Professor of Sociology, University of Pennsylvania. He is currently investigating missing-data techniques, the analysis of change scores, and the effect of mobility on scientists' productivity. His *Event History Analysis* appeared in 1984.

■ DARRELL STEFFENSMEIER (Relative Cohort Size and Youth Crime in the United States, 1953-1984) is Professor of Sociology, Pennsylvania State University. His current research interests include the age-crime relationship, the lower court judiciary, and the crime/labor market relationship. Recent work includes articles on institutional sexism in the underworld (*Sociological Inquiry*, 1986) and the effects of an aging population on the nation's crime rate (*Journal of Research in Crime and Delinquency*, 1987), and a book on the trade in stolen property (*The Fence: In the Shadow of Two Worlds*, Rowman and Littlefield, 1986). CATHY STREIFEL is a Doctoral Candidate in Sociology, Pennsylvania State University. She is currently engaged in dissertation research which involves a cross-sectional analysis of the status of women in the United States and its relationship to female crime. She is also continuing research on social influence on women's alcohol use. MILES D. HARER is a Research Analyst, Department of Justice, Federal Bureau of Prisons. His current research interests are poverty, income inequality, and crime; racial and ethnic differences in crime rates; and the demography of prison populations.

## EDITOR'S COMMENT

### TWO ISSUES: DOCUMENTATION AND NONSTATISTICAL MANUSCRIPTS

In the June issue of *ASR*, I recommended that data banks and data depositories be carefully described in the References section of published articles, along with other bibliographic sources. Other major scientific journals are also adopting this practice, so it is likely that it will soon become widespread. So far, contributors to *ASR* have readily acceded to the request for thorough documentation of data sources. The referencing of privately owned (or individually worked up) data banks and code books may imply that they are public property available to anyone just for the asking, as any book in the library. Understandably, scholars who have spent much time, often years, in amassing data will be reluctant to offer them unconditionally to anyone. *ASR*'s policy is not intended to force scholars to give their data to anyone before they have had reasonable time to exploit them. Rather, its intent is to make it possible for colleagues to replicate each others' published studies which, by definition, are in the public domain. Authors and replicators, as responsible members of a scholarly community, should be able to work out mutually acceptable ways on how data will be made available and used. A written agreement is advised. A related question that I am not addressing is, What data are in the public domain? NSF has provided guideline answers to this question as have other agencies, and perhaps *Footnotes* should publish these guidelines on regular occasions.

The editorial board of *AJS* has decided that authors must cite in the text and in the References all machine-readable data files (MRDF) used in the preparation of their articles. Authors are also asked to provide details on what software they used with these files. This strikes me as a good practice to follow. *ASR* is not enacting legislation or compulsory rules. It is simply proposing that colleagues do all they can to advance their common research enterprise. I invite you to write me about your thoughts, criticisms, and suggestions.

Past editors of *ASR* have complained that many sociologists feel that *ASR* is only interested in empirical research. In my view, the term is a redundancy. Research, by definition, is empirical. What critics probably mean is that *ASR* editors are biased in favor of articles loaded with statistics. Although I know that past editors have been eager to publish manuscripts of a theoretical, historical, comparative, and qualitative nature, the flow of such manuscripts in these areas has remained small. I believe that editors are mostly at the mercy of secular trends in the discipline. They try to publish the best of what comes in. In a content analysis of two issues of *ASR* in 1936 and each tenth year thereafter, I found support for the existence of these secular trends. Demography, family and marriage, and community were the most published areas in 1946 and 1956, social psychology in 1956, stratification in 1976, and so on. Theory and methodology both peaked in 1956 and 1966. Criminology remained relatively constant over the entire period. Theory, social psychology, and industry-occupations seemed to be on the upswing in 1986. Importantly, since 1956 the use of statistics became the dominant norm. Since 1966, theory testing in subareas of the discipline with the use of statistics also became the norm. Thus, although purely theoretical articles declined after 1966, theory testing emerged as a strong trend. Scholars also began to use statistics in articles dealing with historical, comparative, and other traditional "qualitative" areas. Even though articles varied in the extent of their quantitative or qualitative emphasis, the distinction seemed to be breaking down. I suspect that an analysis of other major journals would reveal *ASR*'s trends and fluctuations.

Of course, editors have marginal influence over what gets published, but it is probably less than most people imagine. This editor, like his predecessors, would love to exercise his marginal and positive influence on manuscripts that have a theoretical, historical, comparative, or qualitative emphasis. So, send them in.

## ERRATA

"Social Differentiation in Criminal Victimization: A Test of Routine Activities/Lifestyle Theories," by Terance D. Miethe, Mark S. Stafford, and J. Scott Long (*ASR* 52:184-94).

We made several errors in our recent paper. None of these errors change our substantive conclusions. First, the difference of chi-square tests associated with Tables 2 and 3 was incorrectly reported.<sup>1</sup> In checking these results, a "bug" was found in version 2.1 of SPSS-X's loglinear program which resulted in slightly incorrect chi-square values. With these corrections, the inclusion of the interactions with lifestyle/activity variables significantly improves the fit over model 2 for both types of victimization (see table). Yet, the lifestyle/activity variables remain more important in explaining property victimization. This can be most easily seen by computing Goodman's (1972) analogous measure to  $R^2$  for dichotomous variables. The addition of the direct effects of lifestyle/activity variables increases the  $R^2$  for property victimization by .142 (.844-.702), but

only by .023 (.866-.863) for violent victimization. Comparing model 2 and model 3, the addition of the interactive effects increased the  $R^2$  by .057 for property crime and by .034 in the case of violent victimization. These tests support our earlier conclusion that lifestyle/activity variables are more important in explaining property victimization. Second, a clerical error was made when tables from an earlier version of the paper were combined. The interactive effects reported in the M3 column of Tables 2 and 3 should be taken to the fourth power. This does not change the direction, relative magnitude, or the statistical significance of the effects. Finally, the predicted activity-specific odds in Table 4 were computed for each demographic configuration, weighted by the proportion of observations within each cell, and then averaged. Thus, for each level of an independent variable, the predicted activity-specific odds represent a weighted average of all demographic configurations involving that level of the independent variable.

### REFERENCES

- Goodman, Leo A. 1972. "A Modified Multiple Regression Approach to the Analysis of Dichotomous Variables." *American Sociological Review* 37:28-46.

Table: Corrected Chi-Squares and  $R^2$  for Loglinear Models of Violent and Property Victimization

	Violent Victimization			Property Victimization		
	$LR\chi^2$	d.f.	$R^2(a)$	$LR\chi^2$	d.f.	$R^2$
Model 0:						
Equal odds for all cells	2616.93	127	—	2394.89	127	—
Model 1:						
Demographic variables only	357.47	122	.863	714.19	122	.702
Model 1 vs. Model 2	58.16	2	—	339.64	2	—
Model 2:						
Direct effects of lifestyle variables added	299.31	120	.886	374.55	120	.884
Model 2 vs. Model 3	90.13	10	—	138.64	10	—
Model 3:						
Interaction effects of lifestyle variables added	209.18	110	.920	235.91	110	.901

(a)  $R^2$  calculations are based on the improvement in fit over Model 0. See Goodman (1972) for the computing formula for these  $R^2$ 's.

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# SOCIAL INEQUALITY AND PARTY MEMBERSHIP: PATTERNS OF RECRUITMENT INTO THE HUNGARIAN SOCIALIST WORKERS' PARTY\*

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*This paper presents a causal model of membership recruitment into the Hungarian Socialist Workers' Party, using unit record data from non-Party sources. The findings qualify both the official view and new class theory by showing two roads to Party membership. The first runs through the occupational structure, where workers, professionals, and technocrats are advantaged over individuals outside socialized production. The second runs through educational institutions, providing an educated few with extra opportunities to enter the Party. Results corroborate earlier conclusions that patriarchal allocation of political privileges continues in Hungary. Despite formal guarantees to the contrary, women are significantly underrepresented in the Party. Intercohort changes in membership recruitment show that older cohorts monopolize key Party positions, suggesting that a gerontocracy has emerged in Hungarian political life. Overall, results lend further evidence to the contention that some crystallization in inequalities has taken place in Hungary in the "second stage" of socialist development.*

The egalitarian experiment of early socialists has taken an oblique turn in contemporary socialist societies.<sup>1</sup> The long awaited "withering

away" of the state, the predicted growth of genuine democracy, and the ultimate abolition of class antagonisms have failed to occur in East European nations despite massive social, economic, and political changes during the twentieth century. The oft-repeated claims by these societies to be workers' states and to be gradually eliminating all classes have been strongly criticized by Left and Right alike.

While it is generally conceded that inequalities exist under contemporary socialism, many studies show that they have *declined* since World War II. In a persuasive treatise, Frank Parkin (1971, pp. 141-43) has argued that during the immediate post-revolutionary years East European states were strongly committed to reducing educational, occupational, and income differentials. This claim has, in large part, been substantiated: income inequalities have decreased radically (Connor 1979, p. 232; Ferge 1979, pp. 159-232), educational opportunities have expanded (Simkus and Andorka 1982, p. 743), and distinctions of prestige between manual and nonmanual occupations have narrowed (Giddens 1973, p. 229). Along with these changes, state policies were implemented in the decades following the war with the explicit intent of restricting intergenerational transmission of social inequalities. Inheritance of wealth was eliminated, and quotas were imposed on educational and occupational recruitment to favor children from working class and peasant families (Simkus and Andorka 1982, p. 743).

preference. We do not mean to imply, however, that these countries are, indeed, *socialist* in the late eighteenth- or early nineteenth-century sense of the term.

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<sup>1</sup> There is a wide diversity of opinion over the most appropriate way of referencing East European societies. Labels range from "bureaucratic collectivism" (Schachtman 1962, pp. 37-60), "managerial societies" (Burnham 1962, pp. 112-38), "rational redistributive systems" (Konrád and Szelényi 1979, pp. 47-60), "transitional societies" (Sweezy and Bettelheim 1970, pp. 3-76), "deformed workers' states" (Trotsky [1937] 1972, pp. 52-56), "dictatorship of the proletariat" (Lenin [1918] 1971, pp. 443-46), "dictatorship of a handful of politicians" (Luxemburg [1928] 1970, p. 391), "dictatorship over needs" (Fehér, Heller, and Márkus 1983, pp. 167-86), "state socialism" (Giddens 1973, p. 271; Lane 1976, pp. 19-62), "state capitalism" (Cliff 1974, p. 271), to "actually existing socialist societies" (Bahro 1978, pp. 17-48). We chose the term, "contemporary socialist," because it is relatively free of ideological

The policies introduced to reduce inequalities were impressive, but short-lived. With the advancement of socialist industrialization and the political consolidation of the new powers-that-be, many of the initially egalitarian policies were gradually phased out. Post-revolutionary attempts at "building socialism" were soon overturned by a "second stage" in socialist development (Kelley and Klein 1986, pp. 211-18), which was marked by the expansion of structured inequalities and the crystallization of new privileges (Ossowski 1963, p. 115; Bauman 1974, pp. 140-47; Nove 1983, pp. 307-10). Contrary to their early ambitions, contemporary socialist societies today demonstrate substantial inequalities in their prestige hierarchy (Treiman 1977, pp. 144-48), patterns of social mobility (e.g., Connor 1979, pp. 106-76), opportunities for educational advancement (Simkus and Andorka 1982, pp. 749-50), and distributions of monetary and nonmonetary rewards (Szelényi 1983, pp. 43-84).

The salience of political inequalities under contemporary socialism has long been recognized. Since the revolutionary seizure of power in the name of the proletariat, disillusioned critics of Soviet-type societies have lamented the growing disparity between a Party elite and an out-manuevered non-Party mass (e.g., Marcuse 1958, pp. 238-42; Sorokin 1959, p. 16). Marked differences in the allocation of resources on the basis of Party affiliation have led many to conclude that the political sphere is central to the stratification system of contemporary socialist societies (Parsons 1954, p. 407; Goldthorpe 1966, pp. 655-58; Bauman 1974, pp. 140-47).

To date, little empirical research has followed the avid interest in political dimensions of East European stratification. This is not surprising, given that, up to now, few attempts have been made to collect data on Party membership independently of the annual surveys made by the Party itself. Customarily, results of these annual surveys are not disseminated among social scientists in unit-record form and, if released at all, are distributed as aggregated tables produced by the Party's official research institutes. "We derive much of what we know about dimensions of political inequality in contemporary socialist societies from such sources.

The pathbreaking empirical work on Party membership is T.H. Rigby's on the Soviet Union, in which he used published Party documents as his main source. Rigby's results confirmed the fears of both ardent anti-communists and disillusioned socialists by demonstrating that, since the 1920s, workers have played "second fiddle to the white-collar-intelligentsia stratum" in the Communist Party of the Soviet Union (1968, p. 413). He also

noted a gradual upgrading in the educational credentials of Party members over time (1968, p. 407), and a monotonic trend towards older membership. Finally, he uncovered consistently sharp differences in the participation rates of men and women, indicating that, in spite of Soviet ideological commitment to the equality of the sexes, women have always been significantly underrepresented in the Party (1968, pp. 359-61).

Studies on other contemporary socialist societies and follow-up research on the Soviet Union have reached similar conclusions about Party membership and social inequality. Official statistics from a wide range of East European societies indicate that membership rates among nonmanual workers have increased dramatically since the early 1950s (Staar 1971; Krejci 1976; Lane 1976; Parkin 1976; Szymanski 1979). This upgrading in the social composition of the Party was paralleled, in almost all countries, by a decline in working class membership. Exceptions are Bulgaria, Yugoslavia, and the Soviet Union, where working class participation continued to rise well into the 1960s (Staar 1971, p. 37; Denitch 1973, p. 116; Szymanski 1979, p. 90).

Undoubtedly, these studies have provided us with a great deal of information on the nature of political inequality in contemporary socialist societies. It is important to recognize, however, that their findings were affected by the limitations of their sources in a number of important ways. For one, data analyzed by these researchers were almost invariably obtained from official publications (e.g., Party newspapers, Party Congress reports, national press surveys, Radio Free Europe, publications by the national radio networks, and the U.S. Department of State Bureau of Intelligence and Research) of dubious quality, validity, and reliability. Moreover, their data were in the form of aggregated tables, which prevented researchers from manipulating the information at their own discretion.

In this context, it is timely to offer an analysis of membership recruitment into the Hungarian Socialist Workers' Party using individual-level data from non-Party sources. The present study improves prior research in two ways. First, our data make it possible to apply methods of multivariate analysis to Party membership in ways that were heretofore not feasible. Second, the nature of our sources allows us to interpret our results with much greater confidence, shedding some light on the reliability of previously published official documents.

#### SOCIAL INEQUALITY AND PARTY MEMBERSHIP IN HUNGARY

The Hungarian Socialist Workers' Party has a complicated and conflict-ridden history (see



Figure 1). It was formed in 1918 by a handful of Soviet-trained political activists under the name *Hungarian Communist Party* (Seton-Watson 1957, p. vii; Unger and Szabolcs 1979, p. 279). In the early months of 1919, it merged with governing Social Democrats into the *Hungarian Socialist Workers' Party*, and won political power for the first time (Unger and Szabolcs 1979, p. 283). In less than half a year of its rule, however, it was faced with military invasion by neighboring countries and growing discontent within its own ranks as conservative elements both inside and outside the Party began to object to its radical political program (Seton-Watson 1957, p. vii). These events led to a major split within the Party and, eventually, to its downfall in 1919. Having been declared illegal by the succeeding conservative regime, the Party disappeared from the official political scene. Some of its members fled to the Soviet Union, while others remained in the country, either to engage in underground activities or to continue their work inside other political organizations (Seton-Watson 1957, p. vii; Lane 1976, p. 131).

The Party did not re-emerge from its officially dormant state until the Communist takeover at the end of World War II, when some of its emigrés returned to Hungary to organize once again.

It began with weak political support (in the November 1945 elections it received only 17 percent of the votes), but as a result of a massive membership drive, a series of pragmatic coalitions, and a forced merger with the Social Democrats, by 1949 (now called the *Hungarian Workers' Party*) it counted over 1.4 million members and had succeeded in establishing its one-party rule (Staar 1971, p. 118; Beck 1973, p. 114; Lane 1976, p. 131). During the 1950s, the Party underwent further changes in its social composition, as it progressed from a brief period of personal autocracy under Mátyás Rákosi, to Khrushchev's massive de-Stalinization program and several alterations in government in 1956 (Seton-Watson 1957, pp. x-xiv; Beck 1973, pp. 117-18; Lane 1976, p. 131). With the fall of the 1956 uprising, however, the Party attained its present form under the leadership of János Kádár. Upon coming into office, Kádár attempted to reestablish the favorable reputation the Party once held by giving it a new name, the *Hungarian Socialist Workers' Party* (*Magyar Szocialista Munkáspárt*, or MSZMP), and purging it of its pre-1956 dogmatic elements (Staar 1971, p. 118).

Once an illegal underground organization, the MSZMP is now the leading political institution in Hungary. It dominates political culture with a

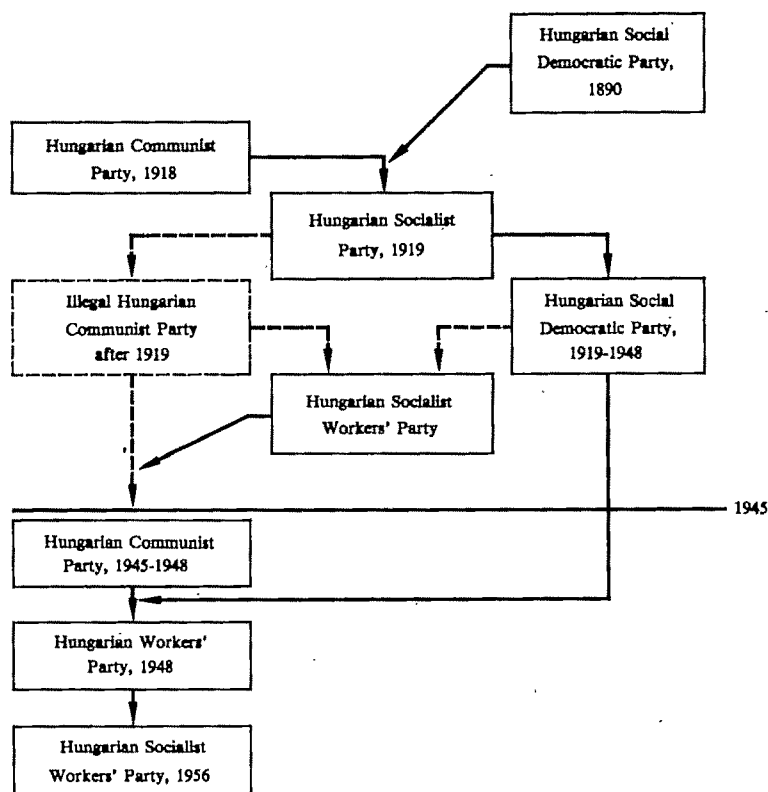


Fig. 1. A Pictorial History of the Formation of the Hungarian Socialist Workers' Party, 1918-1956. Source: This figure is an unabridged version of an illustration in Unger and Szabolcs (1979, p. 389). See text for details.

Table 1. Occupational Structure of Hungary and the Hungarian Socialist Workers' Party in 1977

Occupational Categories	Percent Distribution		
	Hungarian Labor Force	Ever- <sup>a</sup> Members	Members <sup>a</sup> in 1977
1. Managers and leaders	2%	4%	6%
2. Professionals and intellectuals	6	10	12
3. Routine nonmanual workers	18	24	18
4. Skilled manual workers	18	22	19
5. Semi-skilled manual workers	12	7	7
6. Unskilled manual workers	9	4	2
7. Agricultural manual workers	7	6	0
8. Self-employed individuals	4	0	1
9. Others <sup>b</sup>	25	24	36
(Total Number)	(1286)	(248)	(91)

Note: Percentages may not sum correctly because of rounding error. Missing observations were not included in these calculations.

<sup>a</sup> See text for details on the construction of these two variables.

<sup>b</sup> This category includes not-specified wage earners, individuals living off their property and/or private savings, as well as those who were students, retired, or financially dependent in 1977.

unitary ideology and directs all executive, legislative, and judiciary powers (Fehér, Heller, and Márkus 1983, p. 156). It has complete authority over the selection of its own members and it controls many administrative appointments within the larger apparatus of the Hungarian state.<sup>2</sup> Party ratification is often sought in the final selection of candidates to important public positions such as ministerial posts, leading military offices and heads of cultural, educational, and scientific institutions (Bauman 1974, p. 141; Nove 1979, p. 195). By controlling these appointments and circulating personnel between public positions and Party offices, the MSZMP maintains intimate ties with a variety of social, economic, and cultural organizations (Konrád and Szelényi 1979, pp. 211–15).

The rise of the Party to political eminence did not leave its members unaffected. As is well publicized in the Western media, Party members today enjoy definite social, political, and economic advantages. They have the privilege of attending Party schools, shopping at special stores, and vacationing at the most desirable holiday resorts (Connor 1979, pp. 251–53). They are also more likely to receive state-subsidized housing, own a car or vacation home, and purchase meat several times a week (Szelényi 1976, pp. 312–18; Szelényi 1983, pp. 62–64; Kolosi and Bokor 1985, pp. 109–11). Finally, Party membership significantly improves one's chances of obtaining higher status and skilled occupations, favorable locations in

the hierarchy of authority, as well as top decision-making posts (Staar 1971, pp. 118–20; Beck 1973, pp. 113–18; Lane 1976, p. 131).

Our data for 1977 confirm some of these claims. Thus Table 1 shows that MSZMP members are overrepresented in managerial, professional, and skilled manual occupations, while Tables 2 and 3 show that they are also more likely to be found in supervisory and decision-making positions. Of course, rank-and-file membership does not necessarily guarantee these special benefits, nor does it automatically assure greater influence in politics. In other words, it is not equivalent to being a Party functionary, or an *apparatchiks*. At the same time, it holds the key to many opportunities that are not open to nonmembers and is the first step on the ladder to social and political favor in Hungary.

#### THEORIES OF POLITICAL INEQUALITY

Three theories dominate the discussion of political inequalities in Hungary. The first is the orthodox Marxist position, which argues that contemporary socialist societies are characterized by a friendly collaboration of nonegalitarian classes. These classes are not antagonistic or hostile and are securely under the control of the dictatorship of the proletariat (Lenin [1918] 1971, pp. 110–13; Stalin 1947, p. 621). In spite of the manifestly unequal distribution of goods and rewards (Lane 1976, pp. 177–211; Szelényi 1978, pp. 73–79), contemporary orthodox Marxists continue to characterize these societies as workers' states. Göran Therborn, for example, maintains that in the Soviet Union both economic and political institutions are genuinely working-class forms of organization because their leadership has an "ideological bond of solidarity" with the masses (1978, pp. 58–60).

<sup>2</sup> Admittance into the Party is contingent on nomination by two respected members, followed by a trial period during which candidates are expected to demonstrate both their good standing in the community and their commitment to communism.

Table 2. Authority Structure of Hungary and the Hungarian Socialist Workers' Party in 1977

Levels of Authority	Percent Distribution		
	Hungarian Labor Force	Ever- <sup>a</sup> Members	Members <sup>a</sup> in 1977
1. No subordinates	72%	52%	58%
2. 1 to 10 subordinates	16	20	18
3. 11 to 99 subordinates	9	21	17
4. 100 or more subordinates	2	8	7
(Total Number)	(1450)	(268)	(101)

Note: See notes to Table 1.

Along similar lines, Albert Szymanski (1977, pp. 80-91) insists that the Soviet state is truly an instrument in the hands of the proletariat because it allows workers to participate in the making of political decisions, encourages free public debate over fundamental social issues, and recruits its members primarily from the lower class.

In the context of this study, the dictatorship of the proletariat thesis has two possible interpretations. On the one hand, it may be construed as saying that the Hungarian Socialist Worker's Party (even as its name suggests) recruits its members largely from the working class. This rendering of the official view is not only testable, but is also congruent with Szymanski's (1977, p. 91) assertion that Party members are from the lower class. Alternatively, the thesis may be read as saying that while the Party is not, strictly speaking, a workers' party (in that it is not entirely, or even primarily, composed of workers), nonetheless, it represents their *fundamental* interest in the liberation of society from antagonistic class relations.<sup>3</sup> This latter interpretation corresponds to Therborn's claim (1978, pp. 59-62) that although the Communist Party of the Soviet Union is dominated by a cadre elite, this leadership is openly democratic and uniquely representative of working-class interests. Given the overwhelming evidence against Therborn's argument (see Tables 1, 2, and 3 above), we will focus only on the former interpretation of the orthodox position.<sup>4</sup>

The second theory stresses the multifaceted nature of social differentiation in Hungary. Disseminated largely by contemporary academics, it draws heavily on a long tradition of pluralist theory, as well as on Lenski's (1966,

pp. 84-88) notion of "status inconsistency." Originally, this theory says that various dimensions of social inequality are imperfectly correlated in the case of some individuals, such that their rankings on certain dimensions of inequality do not correspond to their rankings on others (Lenski 1966, pp. 86-88). While this approach has come under much criticism in American sociology since the 1970s,<sup>5</sup> a modified version of this theory has only recently emerged in Hungary as a popular and officially acceptable position on social inequality. In its contemporary socialist setting, the theory of status inconsistency no longer pertains merely to the exploration of individual psychology, but rather advances a more general theory of social inequality. Advocates of this thesis maintain that, under contemporary socialism, not only particular individuals, but society at large is characterized by extensive status inconsistencies (Kolosi 1982, pp. 34-40; Kolosi 1984a, pp. 51-103; Kolosi 1984b, pp. 11-72; Róbert 1984, pp. 223-44). It follows from this view that social inequalities in these societies are far less unitary and imposing than in the West because their patterns of advantage and disadvantage are complementary rather than accumulative.

Regarding determinants of Party membership, this thesis is not altogether opposed to the idea that a politically privileged group has emerged in contemporary socialist societies, nor does it deny the possibility that such a group may be reproducing itself over time. It merely suggests that the distribution of political privileges is either independent of other dimensions of social inequality or is the mirror image of these.

Finally, the third account of political inequalities contends that a historically new class has emerged in contemporary socialist societies, a class whose interest in maintaining its current advantages renders it a less-than-complete ally of the proletariat. This account claims that power and privilege in contemporary socialist

<sup>3</sup> For a distinction between *fundamental* and *immediate* class interests, see Wright (1978, pp. 88-91).

<sup>4</sup> It might be argued that, despite the re-emergence of inequalities in these societies, Party leaders are still committed to representing the interests of workers at some abstract ideological level. However, this type of commitment is intrinsically unmeasurable and cannot be tested in this paper.

<sup>5</sup> For methodological and substantive criticisms of these claims, see Laumann and Segal (1977) and Hodge and Siegel (1970).

Table 3. Decision-Making Structure of Hungary and the Hungarian Socialist Workers' Party in 1977

Levels of Decision-Making Power	Percent Distribution		
	Hungarian Labor Force	Ever- <sup>a</sup> Members	Members <sup>a</sup> in 1977
<b>A. Decisions Over Coworkers</b>			
1. None, or not much say	20%	8%	10%
2. Some, or decisive say	80	92	90
(Total Number)	(1076)	(212)	(73)
<b>B. Decisions Over the Workplace</b>			
3. None, or not much say	55%	30%	32%
4. Some, or decisive say	45	70	69
(Total Number)	(1064)	(210)	(70)
<b>C. Decisions in Local Area</b>			
5. None, or not much say	87%	72%	66%
6. Some, or decisive say	13	28	34
(Total Number)	(1076)	(266)	(100)
<b>D. Decisions in the Country</b>			
7. None, or not much say	97%	92%	89%
8. Some, or decisive say	3	8	11
(Total Number)	(1423)	(263)	(97)

Note: See notes to Table 1.

societies are monopolized by individuals controlling collective property (Trotsky [1937] 1972, pp. 248–56; Djilas 1957, pp. 37–69; Cliff 1974, pp. 170–72), as well as by those owning significant amounts of cultural capital (Bahro 1978, p. 147; Gouldner 1979, pp. 18–27; Konrád and Szelényi 1979, pp. 24–35). The implication of new class theory for our study is that the social composition of the MSZMP is by no means self-evident (compare the orthodox position), nor is it of little interest (as suggested by the status inconsistency thesis). On the contrary, it may have far-reaching consequences for the distribution of political privileges, as well as for the general organization of the social structure of these societies.

## METHODS AND RESULTS

### *The Data*

This analysis is based on the *Way of Life, Quality of Life, and Values* survey of the Institute of Sociology in the Hungarian Academy of Sciences. In 1977, the Institute questioned a nationally representative sample of 1,462 individuals about their family origin, life history, educational and occupational careers, family situation, living arrangements, daily problems, and perceived opportunities in life. Unlike most Hungarian surveys carried out independently from the Party, the *Way of Life, Quality of Life, and Values* survey contains several items on Party membership and political activities.

The coding of survey responses took place in several steps since 1977. Unfortunately, many of the original questionnaires were misplaced during a move in which the entire data archive

was relocated in Budapest. As a consequence, several important variables were lost altogether, and others (e.g., father's and mother's occupation) were no longer available for a large enough subsample to be used in this analysis. In spite of this major deficiency, the *Way of Life, Quality of Life, and Values* survey is still one of the best sources of data from Eastern Europe with reliable information on Party membership.

### *Empirical Specification*

Table 4 describes the variables used in the analysis. Two measures of Party membership were derived from the data. The first one, "ever membership," was obtained directly from a question that asked if respondents had ever joined the MSZMP. Without information on membership duration, "ever membership" is a poor estimate of the simple participation rate because it consistently overestimates the number of members in any single year. To correct for this, an additional variable indexing "active membership" in 1977 was constructed from two questions in the survey. This variable disaggregated ever membership into (a) those who were "politically active" in 1977 and (b) those who were no longer "politically active" in that year.<sup>6</sup>

<sup>6</sup> Political activism in 1977 was measured by how often individuals participated in political life. Those claiming to be politically active once or more than once a week were coded as "active members," while those politically active less than once a week were coded as "no longer active." This cut-off point for political activism was chosen because mandatory Party meetings are held once a week.

Table 4. Descriptive Summary of the Variables Used in the Analysis

Variables	Coding	Percentage <sup>a</sup>
Ever-Membership	0. Never member	82
	1. Ever member	18
Membership in 1977 (entire sample)	0. Not active member in 1977 (never joined the Party, or has been a Party member but participates in political life less than once per week)	93
	1. Active member in 1977 (ever member and participates in political life once, or more than once per week)	7
Continued Membership (ever members only)	0. No longer active in 1977 (has been a Party member but participates in political life less than once per week)	62
	1. Active member in 1977 (ever-member and participates in political life once, or more than once per week)	38
First Occupation	1. Working class	85
	Unskilled manual workers	(19)
	Skilled manual workers	(31)
	Routine nonmanual workers	(14)
	Agricultural manual workers	(22)
	2. Not in socialised production	11
	Housewives	(3)
	Helping family members	(7)
	Self-employed	(1)
	3. Professionals and technocrats	4
	Intellectuals	(4)
	Managers and leaders	(0)
Education <sup>b</sup>	1. Primary schooling	50
	Some primary schooling	(47)
	Completed primary school	(3)
	2. Secondary schooling	32
	Apprentice training	(19)
	Vocational high school	(5)
	Academic high school	(8)
	3. Tertiary schooling	18
	Technical college	(5)
	Technical academy	(4)
	University	(9)
Father's Education <sup>c</sup>	1. Less than eight years	49
	2. Eight years or more	10
	3. Insufficient information	41
Mother's Education <sup>c</sup>	1. Less than eight years	63
	2. Eight years or more	30
	3. Insufficient information	8
Gender	0. Male	50
	1. Female	50
Cohort	0. Younger than 45 in 1977	50
	1. 45 or above in 1977	50

Note: See text for details on the coding of these variables.

<sup>a</sup> Unless otherwise indicated, missing observations were not included in the calculation of these percentages.

<sup>b</sup> In the analysis, respondent's education was entered into logistic regressions as a continuous variable with three categories. See text for details.

<sup>c</sup> In the analysis, respondents in categories 3 ("insufficient information") and 1 ("less than eight years") on both father's and mother's education were combined into a single category. This was done because, in an analysis unreported here, we were unable to reject the model that placed an equality constraint on the effect of these categories on party membership.

The effect of first occupation on membership recruitment is of particular significance to this research, given the ongoing debate over the social composition of MSZMP membership. By estimating this effect, we can determine if patterns of recruitment are related to occupational incumbency and, if so, whether they favor workers or professionals and technocrats. To adjudicate between these competing hypotheses, the occupational codes were aggregated into three major categories: the "working class" includes skilled and unskilled manual workers,

routine nonmanual workers, and agricultural laborers; "professionals and technocrats" includes professionals, intellectuals, managers, and leaders; and a residual category ("not in socialized production") includes small independent producers, helping family members, as well as those outside the paid labor force.<sup>7</sup>

<sup>7</sup> In an analysis not reported here, Party membership was regressed on the disaggregated occupational categories presented in Table 4. On the basis of these regressions, we were unable to reject the model, which

Respondents' education was included to assess the claims of new class theorists that technical expertise and specialized skills create greater opportunities for party membership. In an analysis unreported here, education was entered into a set of logit models as a categorical as well as a continuous variable. On the basis of these regressions we were unable to reject the model that suggested that the effect of schooling is linear. Consequently, in the analyses that follow, education will be treated as a continuous variable with three categories (see Table 4 for details).

Lacking conventional data on family background (i.e., father's and mother's occupation), we were forced to rely on parental education as a reasonable proxy measurement. It is true that parental education is a fallible measure of social origins, but its effect in our models represents the combined influence of parental education and occupation, insofar as these two variables are correlated.<sup>8</sup> Throughout the analysis, therefore, parental education will be treated as a proxy for social background.<sup>9</sup>

The first step in constructing our measure of parental education was to code father's and mother's education as dichotomies: respondents whose parents completed eight or more years of schooling were assigned the score of 1; others were assigned zero.<sup>10</sup> The next step was to

combine father's and mother's education into a single continuous variable, "parental education," which was coded 1 if both parents had less than eight years of schooling, 2 if only one parent had less than eight years of schooling, and 3 if both had completed eight or more years.<sup>11</sup>

Respondents' gender was included in the models to determine if prior studies (e.g., Scott 1974) were corrected in suggesting that women are discriminated against in political life. Age was included to ascertain if a system of seniority operates within Party politics. To test the latter proposition, age was coded as a dichotomy: respondents aged 45 years or older in 1977 were coded as 1; others were coded as zero. In our models, the age coefficient can be interpreted not only as a life-cycle effect, but also as a cohort effect. That is, when we treat these two categories as birth cohorts, then we can make some inferences—subject to the usual caveats (Ryder 1965, pp. 848–85; Duncan 1967, p. 59)—about trends over time. Note that the older cohort contains respondents who came of age either prior to the socialist transformation or during the "first stage" of socialist development, and the younger cohort includes those who grew up under the "second stage" of the new socialist regime. Although the life-cycle and cohort interpretations of age are equally compelling, they are also equally indistinguishable. Both interpretations will be discussed throughout the paper.

### Methodology

The first part of the analysis examines the degree to which ever membership in the MSZMP depends on respondents' first occupation, schooling, parental education, gender, and age. In a series of single-equation logit models, the log odds of ever membership were specified to be a function of the five explanatory variables described above. These models took the following form:

$$\log \left( \frac{p_{ij}}{1 - p_{ij}} \right) = \alpha_{j0} + \sum_{k=1}^K \beta_{jk} X_{ijk} \quad (1)$$

where  $p_{ij}$  is the probability that the  $i$ th individual

constrained detailed occupational parameters to be equal within each major occupational category used in this research. Results from these regressions are available from the author on request.

<sup>8</sup> Using information from the 1962–64 national survey of occupational mobility in Hungary, Rudolf Andorka (1982, p. 299) reports this correlation to be .470 for wage-earning men ( $N = 1496$ ) and .529 for wage-earning women ( $N = 1133$ ).

<sup>9</sup> An argument could be made that parental education is a measure of the amount of cultural capital respondents were exposed to inside their families, were we to interpret it as *parental* cultural capital. Unfortunately, our data are far too skewed in the direction of lower education to comfortably allow for this interpretation: only 2 percent of fathers and 4 percent of mothers in the sample proceeded beyond their secondary (high school or technical school) education. Because new class theorists regard only higher educational credentials (e.g., a university degree or some form of specialized technical expertise obtained after the completion of secondary school) as cultural capital, we hesitate to designate parental education as parental cultural capital.

<sup>10</sup> Eight years was chosen as the cut-off point because our sample is made up of respondents whose parents completed their education prior to 1949, when only six years of schooling were compulsory. Since well over half of all parents in our sample never proceeded beyond the compulsory six years, those with eight or more years of schooling (a mere 10 percent of fathers and 30 percent of mothers) were delineated from the rest by the effects of credentialing.

<sup>11</sup> The accuracy of this aggregation was tested in two regressions, neither of which is reported here in detail. In them, party membership was regressed on father's and mother's education, as well as on the combined "parental education" variable. Global tests between the fit of these two hierarchically nested models indicate that the model containing the continuous parental education effect provides a more acceptable fit to the data.

makes the  $j$ th transition (i.e., enters the MSZMP);  $\alpha_{j0}$  is the grand mean;  $X_{ijk}$  is the value for the  $i$ th individual on the  $k$ th independent variable; and  $\beta_{jk}$  are the parameters to be estimated from the data. These models were estimated with 1257 cases. After listwise deletion of missing data, 86 percent of the original sample remained.

The second part of the analysis disaggregates ever-membership into two transitions and carries out cross-transition comparisons of the effects of first occupation, schooling, parental education, gender, and age. The transitions considered include (a) whether respondents had ever entered the MSZMP and (b) given that they had entered, whether they were active members in 1977. This part of the analysis involved three steps. First, two "transition samples" were created from the data. The first included the entire sample of respondents with complete information on all variables ( $n = 1257$ ), while the second contained only those who have successfully completed the first transition ( $n = 237$ ). In the second step, the two transition samples were pooled and a new variable, "continuation," was created to index each transition. This was coded 0 for those appearing in the first transition, and 1 for those who had advanced into the second. Finally, the pooled data were analyzed using a two-equation logit model, which treated Party membership as a single, dichotomous response variable and controlled for membership duration by the inclusion of the "continuation" variable (as well as its interactions) in the model. This model was of the following form:

$$\log \left( \frac{p_{ij}}{1-p_{ij}} \right) = \alpha_{j0} + \sum_{k=1}^K \beta_{jk} X_{ijk} + \sum_{k=1}^K \delta_{jk} X_{ijk} T_{ij} \quad (2)$$

where  $p_{ij}$  is the probability that the  $i$ th individual makes the  $j$ th transition (i.e., either enters the MSZMP, or continues membership);  $\alpha_{j0}$  is the grand mean;  $X_{ijk}$  is the value for the  $i$ th individual on the  $k$ th independent variable;  $\beta_{jk}$  are the main effects;  $\delta_{jk}$  are the continuation interaction effects to be estimated from the data; and  $T_{ij}$  is coded 1 for individuals who have completed the first transition, and 0 otherwise.

#### *The Social Composition of Ever-Members in the Hungarian Socialist Workers' Party*

Table 5 reports the results from three logistic regressions of the log odds of ever membership on the five explanatory variables. Entries (Lines 1 through 7) are maximum likelihood estimates for the effect of a unit change in each independent variable on the log odds of ever membership. Also reported in this table are the likelihood-ratio test statistic,  $L^2$  (Line 8), and the degrees of freedom (Line 9) associated with each model.

The model presented in Column 1 says that variations in membership outcomes are due to differences in the social position of individuals at the time of their first occupation. This model enables us to directly test the orthodox view that workers are the primary beneficiaries of political privileges in Hungary. According to Lines 2 and

Table 5. Social Composition of Ever-Members in the Hungarian Socialist Workers' Party ( $N = 1,257$ )

Independent Variables	Models		
	First Occupation Effects	Full Model	Trimmed Model
1. Constant	-1.370* (.076)	-2.674* (.295)	-2.679* (.287)
2. Not in socialized production	-1.880* (.456)	-1.728* (.459)	-1.728* (.459)
3. Professionals and technocrats	0.734* (.301)	-0.026 (.343)	
4. Education		1.017* (.116)	1.020* (.112)
5. Parental education		-0.522* (.139)	-0.522* (.139)
6. Gender		-0.800* (.164)	-0.799* (.164)
7. Cohort		0.969* (.166)	0.971* (.166)
8. Likelihood-ratio test statistic ( $L^2$ )	1189	1044	1045
9. Degrees of freedom	1254	1250	1251

Note: Entries are maximum likelihood parameter estimates from logistic regression, (standard error). The dependent variable in each model is the log odds of ever joining the Hungarian Socialist Workers' Party. Categorical variables were coded as follows: First Occupation (working class, not in socialized production, professionals and technocrats); Gender (male, female); and Cohort (younger than 45 in 1977, 45 or more in 1977). In all instances, the first category listed inside the brackets serves as the contrast group. Estimates with an asterisk are significant at the .05 probability level.

3, professionals and technocrats are about twice as likely as workers ( $e^{.734} = 2.08$ ), and 13.65 times more likely than individuals outside socialized production ( $e^{(.734) - (-1.88)} = e^{2.614} = 13.65$ ) to enter the MSZMP. These results cast serious doubt on the dictatorship of the proletariat thesis with respect to membership outcomes. However, the model also suggests that a modified version of the orthodox view might be sustained, since workers are substantially more likely to join the Party than individuals outside socialized production (see Line 2).

The remaining models in Table 5 reinforce these conclusions. Column 2, for example, shows that the addition of four exogenous variables leaves the prior conclusions essentially unchanged.<sup>12</sup> Even net of other variables, individuals outside socialized production are still the least likely to become members. They are only one-fifth as likely as workers ( $e^{-1.728} = 0.178$ ), or professionals and technocrats ( $e^{(-1.728) - (-0.026)} = e^{-1.702} = 0.182$ ) to join the MSZMP. Given the descriptive nature of our models, we are unable to shed further light on the concrete mechanisms by which these respondents are discouraged (or prevented) from becoming ever-members. We may only speculate about these causes. One possible explanation is that individuals outside socialized production choose not to join the Party either because they oppose the existing political regime, or because they see too few benefits and too many costs involved in becoming a member. They may calculate that their long-term chances for political posts in Hungary are remote, and the short-term advantages usually associated with Party membership (e.g., promotions, opportunities for further education, vacations at favored Party resorts, etc.) are not directly useful in their lives or jobs. Consequently, the costs involved in time (attending meetings) and money (having to pay membership dues) may not be worth the effort to join. An equally plausible explanation is that the Hungarian powers-that-be have no interest in recruiting such individuals, or they may even deny membership to those outside socialized production.

The coefficient for respondents outside socialized production is virtually unaffected by the inclusion of further exogenous variables, while the corresponding coefficient pertaining to professionals and technocrats was significantly

reduced under the same set of controls (e.g., compare Line 3 across Columns 1, 2, and 3). This implies that the occupational advantage we previously attributed to professionals and technocrats results not from their privileged position in the social division of labor, but rather from their educational credentials (see Line 4, Column 2). For a more rigorous test of this finding, the final model in Table 5 re-estimates the model in Column 2, but constrains the parameter estimates to be the same for professionals, technocrats, and workers. The comparison of the overall goodness-of-fit of these two hierarchically nested models reveals that the more parsimonious model cannot be rejected ( $L^2_3 - L^2_2 = 1$ , and  $df_3 - df_2 = 1$ ).

The coefficient for parental education is surprising. It says that respondents from more-privileged social backgrounds are at a considerable disadvantage in the selection of ever-members: indeed, they are only half as likely as those from less-privileged families to enter the Party ( $e^{-.526} = 0.59$ ). This suggests that while the socialist experiment in Hungary may have failed in bringing to power the "lowest class" (Gouldner 1979, p. 93), it nevertheless managed to allow their children to become a part of the politically privileged few (for a similar conclusion, see Djilas 1957, p. 41; Connor 1979, p. 308). In other words, there is a class bias in party recruitment, but the direction of the bias is actually reversed for the second generation.

The effect of gender on ever membership corroborates Rigby's (1968, pp. 359-61) results by showing that women are approximately half as likely as men to join the MSZMP ( $e^{-0.800} = 0.45$ ). Reasons for this may be quite complex, and it is beyond the scope of this paper to evaluate the merits of each theory of gender discrimination that may apply here.<sup>13</sup> At any rate, this result questions the putatively egalitarian attitude of the Hungarian state towards women and the proposition of the status inconsistency thesis that the distribution of privileges in any one dimension of inequality is largely unaffected by other aspects of the stratification process.

Confirming results from earlier studies on age and Party membership (Rigby 1968; Beck 1973), our model shows that respondents belonging to the older cohort are almost three times as likely as those of the younger cohort to have ever joined the MSZMP ( $e^{0.971} = 2.64$ ). Two alternative theories may account for this result. Were we to regard the coefficient for age as a life-cycle effect, our result says that

<sup>12</sup> In an analysis unreported here, variations by gender, cohort, level of schooling, and parental education in the effect of the five exogenous variables on ever-membership were tested, but no significant interaction effects were found. Results from these analyses are available from the author on request.

<sup>13</sup> For theories of gender inequality in Hungary, see Turgonyi and Ferge (1969), Ferge (1979), Koncz (1982), and Olajos (1983).



individuals are more likely to enter the MSZMP in later stages of their lives. On the other hand, it is possible to argue that the parameter estimate for age measures the degree to which opportunities for ever membership have changed across two distinct periods of socialist development. Under the latter interpretation, our finding supports the contention that political inequalities have ossified in the "second stage" of socialist development so that individuals today have fewer opportunities for ever-membership than they had in the period immediately following the war. In either case, however, the model quite clearly affirms the conventional wisdom of Western scientists that Hungarian political life is dominated by a well-entrenched gerontocracy.

Findings reported so far pertain only to ever membership. The results may differ, however, when we consider the determinants of continued membership. Initial entry, after all, is only one of many hurdles that individuals must clear on their way to prolonged participation in political life. For most people, Party membership is not a life-long commitment; our data indicate, in fact, that as many as 62 percent of ever-members were no longer active in the Party by 1977 (see Table 4). Undoubtedly, some of this decline in participation is attributable to the precarious nature of the MSZMP at various points in Hungarian history.<sup>14</sup> More commonly, however, members leave the Party in one of two ways: (a) they voluntarily exit on account of personal dissatisfaction with the Party (e.g., failure to obtain expected gains from membership or ideological parting of ways), or (b) they are dismissed on account of noncompliance with Party rules (e.g., not attending meetings regularly, or failing to uphold good standing in the community). Given that effective political influence in the Party requires prolonged participation, it may be of some interest to examine not only determinants of ever-membership, but also the dynamics by which long-term members are selected out from the fluctuating mass.

<sup>14</sup> Recall, for example, that for an extended period after the fall the Hungarian Soviet Republic in 1919, the Party was illegal and its activities were closely monitored by the police (Seton-Watson 1957, p. vii; Staar 1971, p. 116; Beck 1973, p. 113; Lane 1976, p. 131). Not until the end of World War II did it become safe to join the Party. Then again during the 1950s, the Party lost many of its supporters due to the notorious pro-Stalinist purges of the Rákosi regime (Seton-Watson 1957, pp. x-xii), as well as to the massive exodus of the critical intelligentsia following the events of 1956 (Fehér and Heller 1983, p. 36).

*The Social Composition of Continued Members in the Hungarian Socialist Workers' Party*

Table 6 reports the results on continued membership, based on the pooled data we described earlier. In these models we are estimating two equations simultaneously: one predicts ever-membership and the other looks at the dynamics of continued participation. This table compares the two models. The first model places an equality constraint on the effect of each explanatory variable across the two equations, implying that the dynamics of selection are identical at both stages of membership recruitment. By contrast, the second model permits the selection process to vary by transition; that is, the effect of each explanatory variable freely varies across the two equations. The comparison of the overall goodness-of-fit of these two hierarchically nested models indicates that the second model provides a significantly better fit to the data ( $L^2_2 - L^2_1 = 30$  and  $df_2 - df_1 = 6$ ). It follows from this that patterns of recruitment into the MSZMP are considerably different from the dynamics that determine continued participation.

The parameterization of the model presented in Column 2 (see Equation 2) shows that the obtained results can be neatly partitioned into the two transition equations. The upper half of Column 2 (Lines 1 through 7) is a replica of the full model on ever-membership introduced earlier (see Table 5, Column 2). The most interesting results are in the lower half of Column 2 (Line 8 through 14). Under the specification of this model, a nonsignificant parameter estimate in the lower half of the column indicates that the variable under consideration has the same effect at both stages of the selection process. Conversely, a significant parameter estimate means that its effect differs by transition.

In light of the above, the interaction term pertaining to professionals and technocrats (Line 10) indicates that, net of other variables, workers, professionals, and technocrats are equally likely to enter the Party, as well as to continue their membership. Our model shows, however, that the significant advantage they have over individuals outside socialized production in the initial recruitment process (see Line 2) disappears entirely by the second stage of selection (Line 9). In other words, individuals outside socialized production are discriminated against only when they enter the MSZMP. Once inside, the early exclusionary barriers against them not only disappear but are actually reversed.

Table 6 shows that while education facilitates membership at both stages of the selection process, its effect is significantly weaker in the

Table 6. Social Composition of Continued Members in the Hungarian Socialist Workers' Party ( $N = 1,494$ )

Independent Variables	Models	
	Main Effects	Full Model
1. Constant	-2.628* (.260)	-2.674* (.295)
2. Not in socialized production	-1.343* (.378)	-1.728* (.459)
3. Professionals and technocrats	-0.150 (.298)	-0.026 (.343)
4. Education	0.838* (.099)	1.017* (.116)
5. Parental education	-0.367* (.120)	-0.522* (.139)
6. Gender	-0.665* (.145)	-0.800* (.164)
7. Cohort	1.026* (.146)	0.969* (.166)
8. Continuation	0.334* (.168)	0.506 (.654)
9. Not in socialized production $\times$ continuation		2.760* (1.056)
10. Professionals and technocrats $\times$ continuation		-0.879 (.722)
11. Education $\times$ continuation		-0.681* (.224)
12. Parental education $\times$ continuation		0.601* (.292)
13. Gender $\times$ continuation		0.687 (.362)
14. Cohort $\times$ continuation		0.346 (.364)
15. Likelihood-ratio test statistic ( $L^2$ )	1363	1333
16. Degrees of freedom	1486	1480

Note: Entries are maximum likelihood parameter estimates from logistic regression, (standard error). The dependent variable in each model is the log odds of membership in the Hungarian Socialist Workers' Party. Categorical variables in the table were coded as follows: First Occupation (working class, not in socialized production, professionals and technocrats); Gender (male, female); Cohort (younger than 45 in 1977, 45 or more in 1977); and Continuation (no longer active in 1977, active member in 1977). In all instances, the first of the codes listed inside the brackets serves as the contrast group. Estimates with an asterisk are significant at the .05 probability level.

second transition (see Line 11).<sup>15</sup> This is somewhat surprising, given the implication of new class theorists that educational qualifications are of considerable importance in the allocation of goods and rewards in contemporary socialist societies. This result, however, cannot be used to discredit new class theories in any general sense, since it probably arises from a historically unique circumstance: the "mass exodus of the intelligentsia from practically all communist parties" after the 1956 Hungarian revolution (Fehér and Heller 1983, p. 46).

The coefficients of parental education show that while respondents from privileged social origins are at a significant disadvantage in terms of initial entry (see Line 5), by the second round of the selection process they have slightly greater opportunities for political advancement

than do offsprings of less-educated families (see Line 12). The remaining two variables in the model demonstrate that the recruitment of MSZMP members is contingent on ascriptive criteria at both stages of the selection process. The effect of gender shows that women are less likely than men either to become ever-members (see Line 6) or to continue their membership (see Line 13).<sup>16</sup> The effect of age demonstrates that older people have a significant edge over younger, both in terms of their chances of initial entry (see Line 7) and their opportunities for continued participation (see Line 14).

<sup>15</sup> Given the successful completion of the first transition, the parameter estimate pertaining to education for those included in the second equation is .336 ( $1.017 - .681 = .336$ ).

<sup>16</sup> It is not possible to reject (a) the hypothesis that gender effects are the same at both stages of membership selection (i.e., in Line 13, .687 does not differ significantly from 0 at the .05 level), or (b) the hypothesis that the effect of gender in the second transition equals zero (i.e., the difference between Lines 6 and 13,  $-.113$ , does not significantly depart from 0 at the .05 level). We would need a larger sample to adjudicate between these two hypotheses.

## CONCLUSION

Our findings indicate that recruitment into the MSZMP is determined by highly structured processes at both stages of membership selection. The systematic effects of first occupation, schooling, parental education, gender, and age on membership outcomes cast some doubt on the status inconsistency thesis. To be sure, these results do not allow us to reject this approach altogether; after all, a literal interpretation of the thesis considers anything less than a perfect correlation between various dimensions of inequality to be favorable evidence. At the same time, the strength of the association between Party membership and other dimensions of social inequality, as well as the direction of these associations, lead us to question this theory's account of membership outcomes. Moreover, given the importance usually attributed to the political sphere in the stratification system of Hungarian society, our findings raise some concerns about the utility of this approach as a general theory of social inequality.

Our results support the suggestion of new class theorists that educational credentials play a significant role in the recruitment of MSZMP members. Obversely, the net disadvantage of workers in the competition for political privileges disconfirms a rigid reading of the orthodox position. At the same time, we have argued that a weakened version of this theory might be sustained in the sense that workers are *advantaged* compared to respondents outside of socialized production (albeit only in the selection of ever-members). Somewhat paradoxically, therefore, our results support both the claims of new class theory and, in some small degree, of Marxist orthodoxy by demonstrating that there are two roads to political participation in Hungary. The first runs through the occupational structure, where workers, professionals, and technocrats are advantaged over individuals outside socialized production. The second runs through educational institutions, providing an educated few with extra opportunities to enter the Party. While the overall pattern is that professionals and technocrats are the most likely candidates for membership in the MSZMP, the opportunities open to workers in the occupational structure should also be emphasized.

In spite of the manifest importance of meritocratic criteria in the recruitment of Party members, we found that ascriptive bases of inequality have a significant effect on the distribution of political privileges in Hungary. Gender differences in MSZMP membership suggest, for example, that some inequalities have proven more resistant to revolutionary changes than expected by some (Engels [1891] 1968, p. 489).

Gender, however, is by no means the only basis of ascription in the selection of Party members. The notable absence of the younger generation at both stages of membership indicates that Hungary is a political gerontocracy. This result is open to two interpretations. On the one hand, the mature character of MSZMP membership may signify that individuals are most likely to enter political life in later stages of their lives. Alternatively, intercohort differences in Party membership may also be interpreted as an indication of the degree to which opportunities for political participation have changed across two distinct periods of socialist development. In other words, our finding supports the contention that political inequalities have rigidified in the "second stage" of socialist development, so that younger cohorts today have fewer opportunities for political participation than had older cohorts in the period following the war. Due to the absence of information on the year respondents entered the Party, we are unable to adjudicate between these two competing hypotheses. We advance both interpretations here in the hope that they may help future research on Party membership.

In sum, we have gained new insights and verified some old conclusions about patterns of recruitment into the Hungarian Socialist Workers' Party. Admittedly, the descriptive nature of our models and the shortcomings of our survey leave many answers unresolved. We hope, however, that our findings will assist future research into this relatively unexplored area of social stratification and shed light not only on the dynamics we have observed in operation, but also on the creation and character of a new political elite in Hungary.

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## SO HAPPY TOGETHER? THE IMPACT OF GENDER SEGREGATION ON MEN AT WORK\*

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*How does gender segregation at work affects men's well-being, as expressed in their psychological orientations toward work? Analyzing a sample of employed males in the 1973 Quality of Employment Survey, we find that men in mixed work settings report significantly lower job-related satisfaction and self-esteem and more job-related depression than men in either male- or female-dominated work settings, even after controlling for individual, job, organizational, and economic determinants of well-being. These findings are difficult to reconcile with theories suggesting that men dislike gender integration at work simply for economic reasons or with the view that male tokens suffer, psychologically by occupying low-status positions viewed as "female" jobs. Rather, our findings are more consistent with perspectives that emphasize how the quality and quantity of intergroup relations decline as groups become more balanced. The implications of our results for segregation theories and for efforts to remedy segregation are discussed.*

The proposition that male workers play a major role in fostering and sustaining gender segregation has been advanced by theorists as diverse as neoclassical economists (Becker 1971), who stress employee "tastes for discrimination," and feminists (Hartmann 1976), who stress "patriarchy." Some theorists have assumed that men's desire to exclude women from their jobs reflects the fear of depressed earnings, while others have suggested that male resistance has more to do with the fear that women would disrupt cohesive and stable work relations among men (Kanter 1977; O'Farrell and Harlan 1982). Yet we know of no empirical studies examining whether men who have female workmates actually evaluate their situations less positively than comparable men in male-dominated work settings. Male-dominated occupations have been shown on average to provide greater economic rewards than female-dominated roles (Treiman and Hartmann 1981; Parcel et al., 1986), but

virtually nothing is known about more affective outcomes that may covary with the gender mix of work settings.

This paper analyzes data from the *Quality of Employment Survey* to explore how gender segregation affects men's psychological orientations toward work. Our interest is not in the determinants of men's mental health per se, but rather in men's reactions to the gender composition of their work setting. By examining how men's perceived well-being is shaped by this feature of their jobs, we hope to gauge male workers' perceived stake in gender segregation. Unlike much previous research on these issues, we examine male attitudes directly, rather than inferring them from their occupational locations or from women's reactions and responses. Moreover, our analyses examine a representative sample of employed men, working in diverse occupations and work settings, while previous work has relied extensively on case studies of single occupations, laboratory experiments, or studies of men employed in highly female-stereotyped positions.

For the sake of brevity, we use the phrase *gender mix* throughout to refer to the gender composition of an individual's work setting. Our research examines the validity of several contrasting perspectives on male workers' reactions to the gender mix. We first review literature relevant to those perspectives and outline our hypotheses. We then report multivariate analyses of the relationships between gender mix and three dimensions of men's psychological well-being at work: job satisfaction, job-related depression, and job-related self-esteem.

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## THEORETICAL PERSPECTIVES ON MEN WORKING WITH WOMEN

*"Women's Work" as Costly for Male Workers*

Some economists and feminist scholars claim that working alongside women involves economic, psychological, or social costs for men. Perspectives that emphasize economic costs contend that women's exclusion from male-dominated settings benefits men economically. Thus, discussions of overcrowding argue that women's confinement to female-dominated jobs restricts the labor supply available for other occupations (Bergmann 1971). This depresses earnings for workers in "overcrowded" (disproportionately female) labor markets, while wages for male-dominated jobs remain higher than they would be in the absence of segregation. This perspective is supported by evidence that female-dominated occupations pay both women and men less than male-dominated ones (Treiman and Hartmann 1981) and that the transition of an occupation from predominantly male to predominantly female is frequently associated with a decline in relative earnings (Reskin and Hartmann 1986, pp. 31-32). Other valued job characteristics, such as skill and autonomy, are also claimed to be less available in female-dominated work settings. For instance, Davies (1975) argued that the entry of women into an occupation is often associated with deskilling.

These disparities in extrinsic rewards and working conditions between male- and female-dominated work settings are presumed to fuel men's resistance to having women enter male occupations. According to this perspective, by affecting men's pocketbooks and working conditions negatively, working alongside women should also lower their utility. This argument, therefore, sees men's interest in segregation as primarily due to its impact on extrinsic rewards; consequently, it implies that controlling for reward levels and working conditions should greatly reduce or eliminate any relationship between gender mix and men's perceived well-being.

Theorists such as Willis (1977), however, have emphasized nonmonetary costs incurred by men who work alongside women. Willis claims that male-dominated occupations affirm masculinity and become imbued with cultural or psychological significance (see also Game and Pringle 1983). Workers in female-dominated settings are thus viewed less favorably than workers in other settings. This assertion is supported by research documenting negative relationships between occupational worth and the percentage of women in an occupation, and by job evaluation studies demonstrating a devaluation of the worth of "women's" jobs relative to otherwise comparable "men's" jobs

and a loss of esteem for people in sex-atypical occupations (for reviews, see McArthur 1984; Bose 1985; Reskin and Hartmann 1986, pp. 15-16). This argument implies that employment in a mixed or female-dominated work setting may threaten men's masculine identities and lower their perceived self-esteem and well-being.

Certain researchers even suggest that the negative psychological and social consequences of working in integrated settings may be more severe for men than for women. Some evidence indicates that there are greater losses of social status for men than for women in gender-atypical occupations (Nilson 1976) and that male tokens have lower self-esteem than female tokens (Macke 1981). Men entering female-dominated occupations, it is claimed, appear to be emulating a lower-status group and, therefore, lose social standing more than do women who enter male-dominated occupations (Hess-elbart 1977).

Feminist theorists (e.g., Hartmann 1976) have extended this argument, suggesting that women in male-dominated work settings also undermine male control in the family and other institutions. Hence, males experience "status contradiction" when working with females as equals, and gender equality at work potentially threatens other patriarchal social structures that benefit men (Hughes 1944). As a result, men possess an interest in segregation at work that reflects their desires to preserve dominance in the larger society (Sokoloff 1980).

All of these perspectives imply that working alongside women has negative consequences for men *independent* of any association between gender mix and extrinsic job rewards; that is, controlling for economic rewards and working conditions might reduce, but should not eliminate, the relationship between gender mix and men's psychological well-being.

*Mixed Work Settings as Costly to Male Workers*

An alternative view treats attitudes as depending on the quality and quantity of interactions in the workplace and not simply on objective job characteristics or rewards. It suggests that males in female-dominated work settings are not necessarily penalized and may actually experience higher well-being. This approach implies that *mixed* work settings will produce least satisfaction for males.

Although men in female-dominated settings may be numerical "tokens," they may not experience the detrimental effects that have been described for female tokens (Kanter 1977; Spangler et al. 1978). Recent research indicates that tokenism results in more positive outcomes

for men than for women. For instance, Fløge and Merrill (1986) found that male nurses had more egalitarian interactions with male physicians than did female nurses. Male tokens may therefore enjoy an informal status advantage over their female workmates and possess greater responsibility and authority (also see Schreiber 1979; Fairhurst and Snavely 1983; Crocker and McGraw 1984; Baron and Bielby 1985, pp. 241–44). Furthermore, the relative deprivation literature suggests that the appropriate comparison group for men in female-dominated work settings may be their female workmates (for reviews, see Merton and Kitt 1950; Davis 1959; Gurr 1970). When men compare themselves to women peers, they should express relatively high levels of psychological well-being because: (1) male tokens are likely to receive superior treatment in the workplace and thus perceive themselves as better off than their female counterparts; and (2) these men enjoy privileges within the larger society associated with the master status of being male.<sup>1</sup>

Other research also suggests that male tokens may have more and better interactions with female workmates than would occur under a different gender mix (Blau 1977, 1980). If men constituted a larger percentage within the workplace, there could be stronger in-group versus out-group pressures. Consequently, token males are more likely to interact well with females than will men in settings where the gender mix is more balanced. South et al. (1982, 1983) analyzed male and female workers in a federal bureaucracy and provided evidence that concurs with this view. If these predictions are correct concerning how male interaction patterns with women vary with the gender mix, they may also have relevance for how the gender mix affects men's job-related well-being. In short, female-dominated work settings may not threaten men's sense of well-being as much as "male cost" arguments assert. Although male tokens have fewer opportunities to interact with men and gain the economic rewards available in male-dominated settings, there are advantages associated with men's employment in female-dominated work settings. Not only are male tokens likely to be treated more favorably than their female workmates, but men's minority status should increase the frequency and quality of male-female interaction. This implies that men in female-dominated settings might per-

ceive themselves as fairly well off, relative to other men.

In contrast, mixed settings may pose the greatest threat to men's perceived well-being. Research on racial hostility and discrimination has indicated that minority-majority relations deteriorate as the ratio between the two groups approaches parity (Allport 1954; Blalock 1956, 1967). Competition for resources and rewards was presumed to increase as minorities gain greater representation. On the other hand, when minority members represented only a small percentage of the total, intergroup relations were more harmonious and discrimination less severe. This prediction from the race relations literature is consistent with research on male-female interaction by South et al. (1982, 1983), which found that male *and* female tokens had more and better interactions with the dominant group than when the sexes were more numerically balanced. This implies that men in mixed settings have fewer opportunities than men in female-dominated settings to interact with and receive support from female workmates.

Of course, opportunities for males to associate with other men are also greater in mixed settings than in female-dominated settings, and one might therefore expect men in mixed settings to be relatively satisfied. However, for reasons outlined above, gender balance in a work setting may lower the availability of certain rewards to men and dampen expressions of male solidarity, compared to all-male settings. Thus, men in mixed settings do not reap the benefits available at either extreme of the gender mix. Accordingly, if findings from the race relations literature apply to gender relations, men should view female workmates as competitive threats most when the gender mix is balanced.

These perspectives provide a more complex view of the relationship between gender composition and men's attitudes than either the "male cost" approaches or Kanter's (1977) theory of tokenism. They imply a curvilinear relationship between gender mix and men's psychic well-being.

## HYPOTHESES

### *Effects of Gender Mix on Well-Being*

This review of the research inspires several hypotheses. First, economic-based approaches imply that men's well-being decreases linearly as the percentage of women in the work setting increases. However, there should be little, if any, net relationship once differences in the availability of extrinsic rewards and desirable working conditions across work settings are held constant. In contrast, other "male cost" approaches emphasize psychological and social

<sup>1</sup> Whether male workers in female-dominated work settings actually compare themselves to female workmates has not been firmly established, however. Moore (1984) argues that both men and women evaluate labor market outcomes on the basis of same-sex comparisons, even when they have coworkers of the opposite sex.



costs that men associate with having female workmates. These perspectives imply that men's well-being declines monotonically with the percentage of female coworkers, even after controlling for differences in rewards and working conditions.

Interaction-based and intergroup conflict approaches also predict the highest well-being among men in male-dominated settings. However, in contrast to male cost arguments, these perspectives hypothesize that men in mixed work settings will experience the lowest levels of well-being, while men in predominantly female settings should view themselves as relatively well off.

### *Interaction Effects*

We also developed hypotheses about interaction effects—that is, those circumstances under which having women workmates should be particularly salient for men's psychic well-being. We expect gender-balanced work settings to lower men's well-being most when the job requires interdependence or trust, since this places a greater premium on fitting in with workmates (Kanter 1977). For similar reasons, we predict stronger negative effects of female workmates on men whose jobs require extensive on-the-job training and among men employed in smaller organizations, since both of these characteristics of the work setting make it harder for men to avoid interacting with women coworkers. We also expect that having women workmates will lead to greater psychological discomfort for men who lack job security. Moreover, since craft unions have been claimed to encourage men's exclusionary behavior (Milkman 1980), women coworkers should lower satisfaction levels more among members of craft unions, which have been characterized as less egalitarian and democratic. We expect that middle-aged men will report less well-being in mixed or female-dominated work settings than older or younger men. The middle-aged men may be more conservative than the young, and they may also be competing more intensely with women for promotions than older males are. Finally, women workmates should be least psychologically detrimental to men whose own wives work.

## DATA AND METHODS

### *Sample*

We analyze data from the 1973 *Quality of Employment Survey* (hereafter QES), a cross-sectional, nationally representative survey of people in the U.S. labor force. These data were collected in January-February, 1973 by the Institute for Social Research (ISR) at the

University of Michigan through personal interviews with 1,496 people living in households in the continental United States. The sample included all household members 16 years of age and older who were employed for pay at least 20 hours per week. The sample is representative of the population of employed workers who met these eligibility criteria (Quinn and Shepard 1974). Our analyses eliminate farm laborers and farm foremen, as well as self-employed men (for whom the gender composition of the work setting is ambiguous). These restrictions resulted in a sample of 822 men with non-missing data on the gender mix (see below).

In ongoing work, we are undertaking comparable analyses for women to determine whether the psychological effects of gender mix differ by sex. We are also conducting longitudinal studies to unravel the causal relationships between gender mix and workers' attitudes. Obviously, workers' attitudes may play a role in sorting people into work settings that vary in their gender mix. Consequently, cross-sectional relationships unearthed in this study do not conclusively prove that the gender mix causes men's well-being.

### *Operationalization*

*Dependent variables: psychological orientations toward work.* To explore whether men are affected psychologically by the gender composition of their work setting, we relied on several attitudinal scales developed by ISR. The key dependent variable is *general job satisfaction*, a scale composed of responses to five job satisfaction questions.<sup>2</sup> We also examined male workers' *job depression* and *job-related self-*

<sup>2</sup> The questions were: (1) "If you were free to go into any type of job you wanted, what would your choice be?" (5 = same as respondent has now; 1 = respondent would retire, not work, or choose some job other than present one); (2) "All in all, how satisfied would you say you are with your job?" (5 = very satisfied, 3 = somewhat satisfied, 1 = not too satisfied or not at all satisfied); (3) "In general, how well would you say that your job measures up to the sort of job you wanted when you took it?" (5 = very well, 3 = somewhat, 1 = not very well); (4) "If a good friend of yours . . . was interested in working in a job like yours for your employer, what would you tell him?" (5 = strongly recommend, 3 = have doubts about recommending it, 1 = advise against it); and (5) "Knowing what you know now, if you had to decide all over again whether to take the job you now have, what would you decide?" (5 = take same job without hesitation, 3 = have some second thoughts, 1 = definitely not take the job). ISR averaged respondent's scores on the five items and multiplied the total by 10, so scale values are integers between 10 and 50. Other job satisfaction studies have used similar measures (e.g., Kalleberg 1977; Kalleberg and Griffin 1978).

*esteem*.<sup>3</sup> We also conducted supplemental analyses of several other attitudinal variables and refer to them below.<sup>4</sup>

*Independent variables: gender composition of the work setting.* Our sample permitted only indirect measurement of the gender mix in each respondent's work setting. National data from the 1970 *Census of Population*, which describe the percent female in each respondent's particular three-digit occupation by three-digit industry combination, were merged into each case in the QES file. These data capture differences across detailed industries in the gender composition of particular detailed occupations. We grouped men into the following categories of work settings: all male (less than 5 percent female); predominantly male (5–19 percent female); mixed (20–70 percent female); and predominantly female (greater than 70 percent female).

However, a particular occupation-industry cell could appear mixed according to the census data we used, but actually consist of segregated settings, either because men are concentrated in different *establishments* than women or because, within a given enterprise, the sexes are segregated by *specific job classifications*. Bielby and Baron (1986) have provided evidence of this type of segregation. Accordingly, we relied on their data to identify seemingly mixed work settings that might actually be highly segregated

either within or between establishments. Their data describe the gender composition of jobs in a sample of California work establishments. They computed within- and between-establishment segregation indices for three-digit occupations in the third edition of *Dictionary of Occupational Titles (DOT)*. Fortunately, those same three-digit DOT codes were contained on the QES file. Consequently, we used Bielby and Baron's results to determine particular DOT occupations that appear mixed (i.e., have between 20 percent and 70 percent female), but that actually exhibited very high within- or between-establishment segregation (indices of dissimilarity greater than 80) in their sample. Among QES men in the "mixed-work setting" category, if a respondent's DOT occupation showed high within- or between-establishment segregation in the Bielby and Baron study, we assigned him to a separate category of gender mix: "mixed-but-segregated" settings.

For example, according to the 1970 Census, 43 percent of "buyers" in "miscellaneous retail stores" were women, giving the appearance of a job with mixed work settings. The DOT code for the corresponding three-digit occupation ("purchasing management occupations") occurred in 93 job titles within 63 establishments in the Bielby and Baron sample (with 17 establishments having more than one job title with this DOT code). We were able to compute a between-establishment segregation index across the 63 organizations having this DOT occupation, as well as a within-establishment segregation index averaged across the 17 organizations having two or more job titles in this DOT occupation. Not surprisingly, both indices were well above 80, indicating that purchasing management occupations are highly segregated by job titles and across organizational settings, so we assigned men in this DOT occupation to the mixed-but-segregated category. This revision to our measure results in five, rather than four, categories of gender mix: all male (less than 5 percent female); predominantly male (5–19 percent female); mixed-but-segregated (20–70 percent female, high within- or between-establishment segregation); mixed (20–70 percent female, low within- and between-establishment segregation); and predominantly female work settings (greater than 70 percent female).

This correction is admittedly imprecise. For instance, the Bielby and Baron data on California establishments are not perfectly representative of national labor markets, and some highly segregated industries were not included in their sample (see also Cain and Treiman 1981). Furthermore, men and women in a seemingly mixed work setting could, in fact, be segregated in different locales, reducing contact, which we

<sup>3</sup> ISR created measures of job-related depression and self-esteem based on workers' responses to questions about how they see themselves in their work. Job-related depression was measured by a scale combining four items on how the respondent feels when thinking about himself and his job: (1) "I feel downhearted and blue"; (2) "I get tired for no reason"; (3) "I find myself restless and can't keep still"; and (4) "I am more irritable than usual". Each of these items was coded: 4 = often, 3 = sometimes, 2 = rarely, 1 = never. ISR averaged respondents' scores on these four items and multiplied the sum by 10; thus, the depression scale consists of integers from 10 to 40. Job-related self-esteem was based on four items asking respondents how they saw themselves in their work: happy/sad, successful/not successful; important/not important; doing their best/not doing their best. Each item ranged from 1 to 7, where 1 indicates a self-perception of not being happy, successful, important, or doing one's best. ISR averaged respondents' scores on the four items and multiplied by 10; thus, the self-esteem scale consists of integers from 10 to 70. For additional details, see Quinn and Shepard (1974).

<sup>4</sup> A key dependent variable of initial interest was the worker's *orientation toward coworkers*, since various theories suggest that men view female workmates as disruptive to workgroup relations. However, the QES questions concerning coworkers were ambiguously worded and did not encourage respondents to construe coworkers in the same sense of "peers" or "status equals" that is used in much of the relevant theoretical literature. Consequently, we have not pursued detailed analyses of the QES items assessing attitudes toward coworkers.

could not measure. Also, for 14 percent of the men in seemingly mixed settings, we had no corresponding data from the Bielby and Baron study on the segregation of their *DOT* occupation. These men were left in the mixed category, even though their work settings might actually be segregated.

Nonetheless, our measurement procedure is certainly better than simply assuming that the percent female in a worker's occupation-industry cell accurately reflects the gender composition of his particular work setting for men in seemingly integrated settings. In addition, any errors we made in failing to assign men to the mixed-but-segregated category should make it *harder* to discover the hypothesized effects of gender mix. Thus, our estimates of statistical effects are conservative, since measurement errors are working against us.<sup>5</sup>

All-male settings contained 51 percent of the men in our sample, most of whom were blue-collar trade workers, such as carpenters, mechanics, plumbers, and truck drivers. The mixed but segregated category (16 percent) contained a diverse set of occupations, including some men in accounting and health jobs, secondary school teaching and administration, assembly work, and food preparation occupations. Predominantly male work settings, employing roughly one-quarter of our sample, also included both white- and blue-collar occupa-

tions, such as insurance agents and other salespersons, machine operators, custodial workers, and a number of public-sector managers and administrators.

Mixed and all-female settings were the smallest categories in our sample. Mixed settings employed 7 percent of the sample, including bank officers and financial managers, postal employees, production clerks, and elementary school teachers. These positions seem generally representative of the detailed occupations that appear integrated based on national labor force data (Bureau of Labor Statistics 1986). Only 2.4 percent of our sample were employed in predominantly female settings.<sup>6</sup> A number of these men worked as operatives, particularly in apparel industries; a few were in bookkeeping and clerical jobs, several were art teachers, and some were apparel salespersons. (The three remaining men were a speech therapist, a credit agency manager, and an orderly.) Men in female-dominated settings in this sample generally *were not* in the most female-stereotyped work roles, such as secretary, hair dresser, or nurse.

One final assumption of our measurement procedure is that the gender mix of people doing the same type of work (occupation) in the same context (industry) is psychologically relevant to men in calibrating the gender balance at work. When men and women are in the same work group, but occupy discrepant statuses (e.g., doctors and nurses on a surgical team), we suspect that gender balance is not problematic. Rather, gender balance is potentially perceived as troublesome by men when women are doing the same kind of work in a comparable context. We believe our use of data on the gender composition of detailed occupations and industries approximates that notion.

*Independent variables: other determinants of psychological orientations.* To ensure that associations between gender mix and men's dispositions are not spurious, we controlled for individual, job, and organizational characteristics that past research has shown to affect job-related well-being (e.g., Kalleberg 1977; Miller 1980; Hodson 1984; Hodson and Sullivan 1985). We controlled for the following individual characteristics: race (1 = white;

<sup>5</sup> As one reviewer noted, Bielby and Baron's data do not always provide sufficient information to determine reliably whether a man in a seemingly mixed occupation is actually in a segregated setting. For instance, his particular job may not have occurred in their sample or else it may have occurred in too few settings to make a reliable guess of its gender mix. If we have incorrectly allocated these individuals to the mixed category, rather than to the mixed-but-segregated category where they might belong, these errors should diminish the negative effect of mixed work settings on men's well-being that we have hypothesized. Various sensitivity analyses suggest that this is indeed the case. We analyzed the data excluding those men in seemingly mixed work settings for whom we lacked reliable data from the Bielby/Baron study on the extent of within- and between-establishment segregation. The negative effects associated with being in a mixed work setting, which we report below, were even stronger. (In one particularly stringent re-analysis, we treated each man in the "mixed" category as a missing case unless we had an estimate of within- and between-establishment segregation for his *DOT* occupation based on three or more organizations in the Bielby/Baron sample. The results were consistent with those reported below, although the significance levels of effects associated with the mixed category were altered somewhat by the loss of cases in that category.) These supplementary analyses confirm that errors in our procedure for differentiating between the mixed and mixed-but-segregated categories seem to work *against* finding the pattern of results we actually obtained.

<sup>6</sup> The small number of men in this category clearly limits our ability to derive precise statistical conclusions about men in these settings. However, Kanter and others suggest that the dynamics of tokenism are most visible in settings where the gender mix is highly skewed. We could have defined predominantly female settings according to a looser criterion (e.g., more than 50 percent female), which would have increased the number of men in the category, but we believe doing so would have made the category less meaningful theoretically.

0 = nonwhite); age (a linear term and a term measuring squared deviations from the sample mean); education (dummy variables denoting high school graduates, those having some college, and college graduates, with the omitted category representing those with less than a high school degree); region (1 = south; 0 = non-south); and family situation (dummies for single, married to housewife, and married to working wife, with the latter category omitted), which has been shown to affect men's mental health (Kessler and McRae 1982). We also controlled for characteristics of the job and work setting likely to affect men's job-related well-being: the level of job training time required (third edition *DOT* ratings of "specific vocational preparation" in the job, ranging from 1 to 9); whether the job requires physical effort, ranging from 1 (not at all) to 4 (a lot); whether the job exposes the respondent to physical dangers or unhealthy conditions (1 = yes; 0 = no); membership in a craft or other union;<sup>7</sup> tenure on the job and in the firm, ranging from 1 (less than one month) to 8 (more than twenty years); steadiness of the work (1 = steady; 0 = not steady); hours worked on the job during the week prior to the survey; and organizational scale.<sup>8</sup>

We included two job characteristics that might be viewed as intrinsic rewards: autonomy (whether the job allows the respondent freedom to do his work), ranging from 1 (not at all) to 4 (a lot); and authority (a dichotomy indicating whether the respondent supervises others). Some studies caution against including job characteristics as regressors in job satisfaction models, since job satisfaction itself determines workers' perceptions of job characteristics (O'Reilly et al. 1980; Caldwell and O'Reilly 1982). Accordingly, our independent variables only include measures of those job dimensions that seem relatively immune from such perceptual biases. While disgruntled workers may misreport some features of their jobs, we trust their responses to

questions concerning whether they supervise others and have autonomy on the job.

Finally, we measured three extrinsic job rewards likely to affect a worker's frame of mind: respondent's earnings, respondent's occupational status, and spouse's earnings. Earnings were measured as the natural logarithm of annual income from the primary job (before tax deductions). Spouse's earnings were measured as the natural logarithm of the difference (before deductions) between total family income and the respondent's earnings. (This variable was coded 0 for unmarried men and those in single-earner households.) Occupational status was measured by the 1970 "male" SEI score for the respondent's three-digit census occupational category (Featherman and Hauser 1977, pp. 320-9).

## RESULTS

### *Descriptive*

Table 1 describes differences in psychological orientations and several key independent variables among men in all-male, predominantly male, mixed but segregated, mixed, and female work settings. As expected, there are significant differences with respect to characteristics of incumbents' occupations.

First, the five work settings differ in how much earnings and occupational status they provide. Men in predominantly female work settings earn significantly less than other men, while men in mixed or all-male settings receive the highest wages. Occupational status, like income, is highest within mixed work settings. However, men in female- and male-dominated work settings receive lower scores on this variable. While the relatively high pay and status of the 59 men in mixed work settings may seem surprising, a number of men in this category were well-paid postal or bank employees. A few were extremely well paid salesmen, and several were teachers, psychologists, or counselors. All-male work settings thus do not invariably provide the highest rewards and most desirable working conditions, as male cost perspectives assume. In fact, settings containing both men and women tend to have somewhat higher earnings and occupational status for men in this sample.

While female settings provide lower earnings and require less vocational preparation, as shown in previous research, they do not offer uniformly low rewards or poor working conditions, and they resemble all-male settings in a number of respects. According to Table 1, men in all-male and female settings express higher levels of job satisfaction, although these zero-order differences were not statistically significant. A similar pattern is observed for job-

<sup>7</sup> The 1973 QES did not ask workers to identify a specific union by name. We classified men as members of craft unions if they were union members and were employed in construction, the railroad industry, a craft occupation (as designated by 1970 census occupation codes 401-575), or as police officers, firefighters, or sheriffs. Other men belonging to a union were categorized as members of noncraft unions.

<sup>8</sup> ISR asked workers, "About how many people work for your employer at the location where you work?—I mean all types of workers in all areas and departments." Respondents selected from the following categories: (1) 1-19; (2) 10-49; (3) 50-99; (4) 100-499; (5) 500-999; (6) 1000-1999; (7) 2000+. These values approximate a logarithmic scale and we retained them since it seems reasonable to hypothesize that organizational size decreases job satisfaction at a decreasing rate.

Table 1. Descriptive Statistics and Analysis of Variance for Variables by Gender Mix of Men's Work Setting

Variables	Total Sample (N=822)	Gender Mix <sup>a</sup>					$\eta^2$
		Male (N=416)	Mixed- but- segregated (N=131)	Predominantly Male (N=196)	Mixed (N=59)	Female (N=20)	
Job satisfaction	37.67 (10.06)	38.05 (9.96)	38.45 (9.95)	38.88 (10.12)	35.80 (10.71)	38.00 (9.88)	.57
Job depression	19.62 (5.88)	19.36 (5.78)	19.50 (5.56)	20.11 (5.84)	20.57 (8.75)	18.17 (5.88)	.61
Job-related self-esteem	59.62 (9.75)	60.15 (9.45)	60.22 (9.59)	58.62 (10.23)	57.96 (9.02)	59.05 (13.57)	.67
Race	.91	.92	.90	.90	.88	.90	.31
Age	37.70 (12.87)	37.75 (12.72)	35.92 (13.01)	38.29 (12.57)	39.32 (13.95)	37.50 (14.58)	.47
Squared term for age <sup>b</sup>	165.46 (183.05)	161.57 (176.82)	170.57 (177.08)	157.76 (181.80)	194.60 (188.89)	202.05 (305.50)	.36
High school graduate	.36	.41	.29	.33	.27	.40	2.19**
Some college	.20	.16	.21	.26	.25	.20	1.04*
College graduate	.18	.12	.34	.17	.29	.25	4.81*
Unmarried	.17	.14	.27	.11	.29	.35	3.23***
Married to housewife	.57	.63	.38	.60	.54	.40	3.46***
Wife works	.26	.23	.35	.29	.17	.25	1.35**
Lives in south	.32	.36	.29	.28	.24	.40	1.00*
Training time (SVP)	5.84 (2.05)	6.01 (1.97)	5.89 (2.03)	5.50 (2.20)	5.98 (1.82)	4.63 (2.14)	1.90***
Danger	.51	.63	.40	.42	.20	.40	7.13***
Physical demands	2.57 (1.05)	2.72 (1.02)	2.37 (1.12)	2.45 (1.05)	2.31 (1.00)	2.55 (1.19)	2.41***
Craft union	.13	.24	.02	.03	.02	.00	10.00***
Other union	.26	.17	.35	.30	.48	.35	4.79***
Job tenure	4.61 (1.82)	4.67 (1.85)	4.51 (1.74)	4.53 (1.78)	4.67 (1.90)	4.65 (1.76)	.17
Employer tenure	5.14 (1.88)	5.15 (1.92)	4.98 (1.83)	5.21 (1.85)	5.27 (1.92)	5.00 (1.89)	.20
Steady	.92	.87	.95	.97	.98	.90	3.45***
Hours worked per week	44.75 (9.56)	45.07 (9.33)	43.72 (9.46)	45.26 (10.43)	44.01 (8.33)	42.10 (9.19)	.54
Organizational scale	3.45 (1.95)	3.21 (1.92)	3.88 (2.00)	3.56 (2.00)	3.61 (1.87)	3.89 (2.16)	1.76***
Autonomy	3.22 (.94)	3.18 (.94)	3.35 (.86)	3.20 (.96)	3.24 (.98)	3.20 (.95)	.39
Authority	.48	.46	.50	.49	.56	.45	.30
Log earnings	9.16 (.49)	9.19 (.47)	9.10 (.51)	9.15 (.50)	9.24 (.53)	8.95 (.60)	1.08*
Log spouse earnings <sup>c</sup>	2.43 (3.80)	1.98 (3.55)	3.66 (4.30)	2.61 (3.94)	2.08 (3.76)	2.99 (4.37)	2.48***
Occupational status	41.28 (23.32)	36.42 (21.32)	48.35 (25.16)	44.33 (23.90)	50.38 (22.35)	39.47 (20.59)	5.79***

Note: Standard deviations shown in parentheses, except for dichotomous variables.  $\eta^2$  is the percentage of the total variation that exists across the five gender mix categories. Asterisks indicate that the mean differences among the five groups are statistically significant at the \*  $p \leq .05$ , \*\*  $p < .05$ , or \*\*\*  $p \leq .01$  level (two-tailed test).

<sup>a</sup> See text for definition of categories.

<sup>b</sup> Squared deviation from average age in sample.

<sup>c</sup> Equals zero for men who do not have a working wife.

related depression and job-related self-esteem, with men in male-dominated and female-dominated settings expressing the greatest psychological well-being.

These results are intriguing. Men in predominantly female work settings regard their situation almost as favorably as men in all-male ones, despite significantly fewer extrinsic rewards. Men in female-dominated settings ex-

hibit some characteristics often regarded as typical of women's orientation to work; strong job satisfaction notwithstanding low pay and prestige. This may reflect a rationalization process whereby workers elevate perceived satisfaction, self-esteem, and the like to compensate for the low pay of their jobs, consistent with cognitive theories of "insufficient justification" and its effects on attitudes (Aronson 1972, chap.

Table 2. OLS Regression of Psychological Well-being Variables on Gender Mix, Controlling for Individual, Job, and Organizational Characteristics

Independent Variables	Job Satisfaction (N = 696)	Job-related Depression (N = 695)	Job-Related Self Esteem (N = 692)
Mixed-but-segregated work setting	.07	.40	.17
Predominantly male work setting	-1.65**	1.30***	-1.95***
Mixed work setting	-3.28***	1.59*	-3.09***
Female work setting	.90	-2.01	.78
Race	.53	.86	.19
Age	.07	-.04	.07*
Squared term for age <sup>a</sup>	1.09E-03	-3.29E-03**	2.52E-04
High school graduate	-1.69	.34	-.84
Some college	-.46	.25	.39
College graduate	-.74	.55	-1.19
Unmarried	1.55	-1.11	.58
Married to housewife	3.70**	-1.90*	2.16
Lives in south	1.15	-.61	.85
Training time (SVP)	.55**	-.01	-.10
Danger	-2.27***	1.41***	-1.54**
Physical demands	.27	.16	.92**
Craft union	2.15*	.97	.35
Other union	.70	-.57	-.25
Job tenure	.26	.34	.05
Employer tenure	-.53	.13	.04
Steady	3.96***	.57	.69
Hours	-.03	.03	-.03
Organizational scale	-.60***	.04	-.52***
Autonomy	.99**	-.14	1.43***
Authority	2.75***	-.61	2.76***
Log earnings	2.85***	-1.12*	1.78*
Log spouse earnings	.37**	-.15	.17
Status	.02	-9.71E-03	.02
R <sup>2</sup>	.173	.072	.121
Adjusted R <sup>2</sup>	.138	.033	.084
Standard error of estimate	9.304	5.727	9.269

Note: Metric coefficients are reported. Significance levels denoted by \*  $p \leq .10$ , \*\*  $p \leq .05$ , \*\*\*  $p \leq .01$  (two-tailed tests, except for effects pertaining to the mixed and predominantly male categories, which are one-tailed).

<sup>a</sup> Squared deviation from average age in sample.

4). Since men in female-dominated settings respond in the same way as female incumbents—positive attitudes toward work, despite relatively low earnings and status—this response appears to be determined situationally, rather than by some gender-based calculus of rewards. This interpretation is also consistent with recent research on women in traditionally male craft jobs, who attribute *more* importance to pay and work content than otherwise comparable women clerical workers (O'Farrell and Harlan 1982).

#### Multivariate Analyses

**Job satisfaction.** Controlling for labor market rewards and other job characteristics, men in mixed settings are significantly less satisfied than men in all-male settings (the omitted category), as are men employed in predominantly male settings.<sup>9</sup>

<sup>9</sup> Due to the large number of control variables in our model, the multivariate analyses are based on fewer cases than the descriptive statistics in Table 1. None of the

The satisfaction levels of men in predominantly female and mixed but segregated work settings, in contrast, do not differ significantly from men in all-male work settings (Table 2). In short, as hypothesized by interaction-based perspectives, men in all-male, mixed but segregated, and all-female settings express the highest levels of job satisfaction in our sample. Men in all-male work settings are significantly more satisfied than men in settings having 5–19 percent females, while men in mixed settings are clearly the least satisfied of any group.<sup>10</sup>

independent variables included in our model contained substantial missing data, nor was there any evidence of nonrandom missing data.

<sup>10</sup> Both male cost and interaction-based approaches predict lower well-being among men in predominantly male or mixed settings than in all-male settings (the omitted category). Consequently, one-tailed tests were employed in assessing the effects of these categories, while all other significance tests in Table 2 were two-tailed.

These findings do not support economic theories that claim men prefer to be employed in male-dominated work settings because they offer greater job rewards and more desirable working conditions. The negative effect of working alongside women on men's job satisfaction is actually increased, rather than eliminated, after controlling for differences among men in earnings and objective job characteristics. This is because mixed work settings in the sample have some characteristics (e.g., higher wages) that increase job satisfaction; once these characteristics are controlled, however, the net effect of gender balance is more strongly negative. These results seem consistent with the claims of feminist scholars who argue that working alongside men is psychologically salient to men in its own right, and not merely because of its effects on extrinsic job rewards.

However, employment in a mixed work setting (or one containing only a small proportion of women) reduces men's job satisfaction the most, while men in female settings (or in mixed-but-segregated settings) are actually slightly more satisfied than men in all-male work settings. In other words, there is not a linear relationship between the "femaleness" of a work setting and men's perceived well-being. Male tokens are not significantly less satisfied than otherwise comparable men, perhaps because they assess their satisfaction relative to their female workmates rather than male workers in other settings. The low well-being of men in mixed work settings appears consistent with the claim that intergroup relations are least harmonious when minorities form a relatively large proportion of the total. Under these conditions, competition by women for rewards and resources may be seen by men as more threatening than if females had a smaller relative presence in the work setting.

Several other control variables in our model are also associated with job satisfaction, all in the expected direction. Job autonomy, earnings, spouse's earnings, higher skill requirements, being in a steady job, having supervisory responsibilities, and being a member of a craft union are all positively related to men's job satisfaction. Job satisfaction is also significantly higher among men who were married to housewives than among men with working wives (the omitted category in Table 2), consistent with research by Kessler and McRae (1982). Conversely, job satisfaction is significantly lower among men working in large organizations or dangerous settings.

In supplemental analyses (not reported in Table 2), we tested our hypotheses concerning interaction effects. Due to the small number of cases in certain categories of gender mix, these analyses differentiated three (rather than five)

groups of male workers: those in all-male work settings, those in mixed work settings, and all others (i.e., men in female, mixed-but-segregated, or predominantly male work settings). This approach makes conceptual sense: men in mixed work settings are more vulnerable than other groups of male workers to the negative effects of working alongside women. Each of the other work settings with female workmates involves some characteristic that buffers male incumbents from the presumed negative effects of women peers. Male tokens in female settings are likely to receive privileged treatment; men in the mixed-but-segregated category are insulated from competition and contact with women coworkers; men in the predominantly male category should not be severely threatened by a small minority of female workmates. (For the sake of brevity, we refer to this latter category of work settings as "intermediate.") As before, working in an all-male setting represented the omitted category.

Overall, there was little support for our hypotheses regarding interactions between gender mix and various job characteristics. Although a few effects were statistically significant, most were in the opposite direction of what we had hypothesized. For example, the negative effect of being in a mixed work setting was stronger in large (rather than small) organizations ( $b = -1.653$ ,  $p < .05$ , two-tailed test). This finding may indicate that formalized personnel policies and work procedures, which are characteristic of large organizations (Blau and Schoenherr 1971; Child 1973), constrain men in mixed settings from simply avoiding interactions with female workmates.

Significant results were obtained, however, for interactions concerning men's family situations. Being in a mixed work setting had a positive effect on the satisfaction levels of unmarried men ( $b = 16.136$ ,  $p < .01$ , two-tailed test) and men whose wives were not employed ( $b = 16.402$ ,  $p < .01$ , two-tailed test). Stated differently, job dissatisfaction in mixed work settings was highest among men whose own wives worked. Similarly, among men in the "intermediate" category, satisfaction was also significantly lower among those with working wives than among those married to housewives ( $b = 3.940$ ,  $p < .05$ , two-tailed test).

The finding for unmarried men could reflect the fact that mixed work settings provide opportunities for establishing social relationships with female workmates. It is less obvious why men whose own wives are employed should be least satisfied in mixed work settings; indeed, we had hypothesized the opposite. Kessler and McRae (1982, p. 224) have speculated that "uneasiness about one's wife

working is associated . . . with attitudes about the appropriateness of women working." This interpretation is also consistent with feminist arguments that men resist workplace integration to preserve patriarchal relationships between the sexes. Our results suggest that the threat of female competition in the workplace might be reduced somewhat for men whose family situations conform to a traditional pattern. In other words, challenges to "public patriarchy" are tolerable if "private patriarchy" remains intact (Brown 1981). Recall that these data were collected in the early 1970s, at the peak of the women's movement and when the place of women in the labor force was less institutionalized than now. Women coworkers may have appeared especially threatening to men during this period of changing gender roles.

*Job depression.* All else being equal, employment in mixed settings or in those with only a small proportion of women is associated with greater job depression than employment in all-male settings. Men in mixed work settings report the highest levels of job depression in our sample, followed by men in settings with 5–19 percent females. Men in mixed-but-segregated settings do not differ significantly in their level of job depression from men in all-male settings (the omitted category). Men in female work settings, by contrast, reported *less* job depression than other men. In fact, the difference in job depression between men in female versus all-male settings is about the same (in absolute magnitude) as the difference between all-male and mixed settings. However, given the very small number of men employed in female settings, that effect was not statistically significant.

Job depression is positively related to employment in a dangerous job, negatively related to earnings, and higher among middle-aged. Men with working wives (the omitted category) appear more depressed about their jobs than men whose wives stay home. However, Ross et al. (1983) found that depression among men with employed wives was acute only among men who opposed their wives' employment. Although we have no information about men's preferences in this regard, recall that these data were collected during a period of changing gender roles, when men might have particularly opposed their wives' employment.

In short, these results closely parallel those for job satisfaction. In particular, they reveal that gender mix has an independent, nonlinear effect on men's psychological well-being. Men employed in mixed settings or those containing a small proportion of women reported higher levels of job depression than men in either male- or female-dominated settings. This poses a further challenge to some economic and feminist

theories, which would identify men in female work settings as those most likely to experience job depression.

*Self-esteem.* As in the preceding analyses, men in predominantly male or mixed work settings scored significantly lower on this dimension of psychological well-being than men in all-male work settings. The general pattern is similar to that for the previous two dependent variables. The highest level of job-related self-esteem in this sample are found within all-male and female work settings.

Organizational scale and working in a dangerous job are negatively related to this variable. Men's job-related self-esteem increases with age, autonomy, being in a supervisory position, and having a job that requires physical effort.

*Other facets of well-being.* The specific forms of discontent documented here among men in integrated work settings do not generalize to every aspect of men's perceived well-being. In supplemental analyses (not reported in Table 2), we examined various ISR measures designed to elicit men's reactions to general conditions of their lives, such as overall satisfaction and escapist drinking, finding scant evidence of differences due to the gender mix. These results are not unreasonable. After all, men have many ways of segregating their work experiences from other aspects of their lives. Indeed, our results documented one such process, namely, that traditional family roles may help insulate men from dissatisfaction experienced from working alongside women. It may not be surprising that working alongside women does not affect aspects of men's general well-being.

## DISCUSSION

This paper evaluated alternative theories regarding how the gender mix of men's work settings affects their psychological well-being. Our analyses reveal more support for interaction-based approaches, which trace men's well-being to both the quantity and the quality of their interactions with women coworkers, than for male cost perspectives, which argue that well-being is determined by men's levels of extrinsic rewards or the "femaleness" of a work role.

To be sure, the effects of gender mix on well-being were not statistically overwhelming, and the small numbers of men in mixed and female-dominated settings warrant caution in drawing strong inferences from these results.<sup>11</sup>

<sup>11</sup> We lack statistical power in concluding that the relationship between gender mix and men's well-being is curvilinear. Nonetheless, we still uncovered statistical evidence of curvilinearity in a formal test. We first estimated a model that added a linear term for percent female and a dummy variable for mixed-but-segregated



Furthermore, the data and results reported here are limited in many respects, particularly by being so distant from the workplace relations they attempt to capture. This study also lacked measures of actual male-female interaction patterns and information concerning whether men in mixed settings *perceive* women workmates as a threat (see South et al. 1982, fn. 1).

Despite these limitations, a few factors give us some confidence in our measure of gender composition and the pattern of results obtained. We have controlled for an extensive array of individual, job, and organizational characteristics, minimizing the chance that the effects of gender mix on men's well-being simply reflected omitted variables. Moreover, notwithstanding the subtlety of the processes we have tried to model, the measurement error in our indicator of gender mix, and the notorious difficulty social scientists have had in explaining variation in such psychic variables as job satisfaction, we uncovered consistent effects in the predicted direction across three different facets of men's well-being. We take this as evidence of a substantial link between the gender mix and men's attitudes toward work. We are also encouraged by the fact that our control variables, such as organizational scale and spouse's employment, had effects on men's well-being similar to those uncovered in past studies that relied on other data sets.

Our results challenge certain prevailing accounts of men's stake in gender segregation. We found little support for theories that suggest men avoid working alongside women principally because the presence of females is associated with lower extrinsic rewards and poorer working conditions than in all-male settings. Men employed in female-dominated settings viewed themselves as better off than some economic and feminist theories predicted. Moreover, men in mixed work settings express lower levels of psychological well-being, even after differences in objective job characteristics and rewards are controlled. In sum, our results suggest that any aversion of males to women workmates is not fueled simply by economic considerations, nor

is this aversion present among all groups of men who work alongside women. Our analyses also suggest that the extent to which working in a mixed work setting affects a man's well-being depends on his family situation. This implies that something long known to be true for women workers is also relevant to men: understanding men's work experience requires paying attention to the connections between the workplace and the home (Sokoloff 1980).

We cannot rule out entirely the possibility that self-selection is one part of the story. In exploratory analyses, for instance, we found some weak evidence that the attitudes of men in female work settings were, like the attitudes of women, generally less authoritarian than the attitudes of men in general. In contrast, men in all-male settings were most likely to support the death penalty and more likely than other men to agree with the statement, "An insult to your honor should not be forgotten." Since such attitudes are likely to be formed prior to entering the labor force, these differences may indicate that men are sorted into work settings that differ in their gender mix as a result of pre-existing attitudes and endowments. Along these same lines, one could argue that men in mixed settings are less satisfied partly because they had less pre-employment information about the gender mix of the work setting than did men in female-dominated settings, who may even have sought out "female" jobs deliberately (Barbara Reskin, personal communication). However, recall that the men in female-dominated work settings in our sample were generally *not* in highly gender-stereotyped occupations, suggesting that this reasoning may not apply. Nor do we see how self-selection arguments alone would explain the co-occurrence of fairly high extrinsic rewards with low well-being in mixed settings, or the relatively high satisfaction of men in low paying, "female" work settings.

Our results suggest that some previous theories may have assumed that male workers have too unified and coherent an interest in gender exclusivity. If men possessed such an interest, then presumably their dissatisfaction at work should increase in proportion to the presence of women in the work setting, which we did not find to be the case. Rather, our findings are more consistent with theories that emphasize how the quality and quantity of male-female interaction is constrained by the gender mix and with accounts that view minority groups as most threatening to the majority when the two groups approach numerical balance. If male co-workers play a role in restricting women's job chances (O'Farrell and Harlan 1982), this may have more to do with constraints on interaction and cooperation associated with different gender mixes than with a

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work settings to the control variables in Table 2. This model posits that well-being is a linear function of the gender mix, except for men in mixed-but-segregated settings, for whom a shift adjustment is made. We then estimated a second model that added dummy variables for the other categories of gender mix. An *F*-test for the increment to explained variance associated with those dummy variables indicates the statistical significance of departures from linearity. Even given the small number of cases in the mixed and female categories, these *F*-tests were significant ( $p = .03$ ) for job-related depression and self-esteem, and nearly so ( $p < .12$ ) for job satisfaction. The curvilinear pattern of results reported here thus seems both substantively and statistically meaningful.

unified exclusionary interest among men. Research by South et al. reached a similar conclusion in criticizing past research for "its overemphasis on the subtleties of motivation and its neglect of opportunities as determinants of human behavior" (1983, p. 578).

This study was motivated by our interest in the extent to which male workers might perceive a stake in gender segregation at work, rather than by an interest in the psychic well-being of men per se. Our findings contain grounds for both pessimism and optimism regarding the potential for integrating women into male-dominated settings. On the one hand, our results suggest that interventions aimed at achieving numerical balance, without addressing the quality and quantity of contact among different groups in the work force, might have deleterious effects for both men and women. If integrated work settings do indeed diminish men's perceived well-being on the job, such settings may be associated with male resistance and hostility toward women, factors that have historically served to limit women's job opportunities and diminish their quality of work.

On the other hand, simply demonstrating that males with female workmates may devalue integrated settings does not establish men's willingness or ability to enforce that point of view. Research is needed, for instance, to identify the circumstances under which employers and coworkers allow males to indulge desires for segregated work or inhibit them from doing so. In addition, the relatively small differences we have documented in men's perceived well-being across categories of gender mix may warrant a more optimistic reading of our results. We doubt that these small differences are sufficient to mobilize male workers' hostility and exclusionary behavior toward females in integrated settings.

Even if male workers are dissatisfied with having female workmates, those perceptions may well prove transitory. Studies of racial integration suggest that prejudicial attitudes and exclusionary behavior often abate when opportunities exist for constructive interaction between whites and nonwhites (Stouffer et al. 1949, chap. 10; Harding and Hogrefe 1952; Katz et al. 1958). We hope our ongoing longitudinal analyses, as well as future research by other scholars, will indicate whether there is reason to expect similar patterns of accommodation by men when women enter work settings previously dominated by men.

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## OCCUPATIONAL SEX SEGREGATION IN METROPOLITAN AREAS\*

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*This paper examines sexual segregation in 42 occupational categories in the metropolitan areas of the United States as of 1980. The results indicate that variations in metropolitan area segregation are strongly related to local distributions of occupations (which we view as "structural propensities" to segregate). Net of structural propensities and of considerable explanatory importance, however, are a number of indicators of population size, economic vitality, and women's competitiveness. The paper concludes by viewing organizations within metropolitan area environments, and urges that variables pertaining to metropolitan areas and to organizations be combined into a single model.*

Over the last 30 years, the participation of women in the American labor force has dramatically increased for all age groups and marital status categories (Maret 1983). There seems to have been a corresponding decline in the sexual segregation of the labor force during the 1970s (Beller 1984), but the degree of overall decline in occupational sex segregation varies somewhat among studies. These variations are apparently due to investigators' reliance on different samples of occupations, different time intervals, and different measures of segregation (Jacobs 1986; Reskin and Hartmann 1986; Treiman and Hartmann 1981; De La Vina 1985). Irrespective of these disagreements about the slope of the trend line, however, there is consensus that the American occupational structure is characterized by a high degree of sexual segregation.<sup>1</sup>

Any widespread, persistent pattern of segregation in a basic institution in American society warrants sociological attention, but the theoretical importance of sexual segregation in the labor force is enhanced by its apparent implications for the stratification system. Among blue-collar workers, for example, industries with the highest concentrations of female workers also have the lowest average salaries (Bibb and Form 1977; Treiman and Hartmann 1981). More generally, Mellor's regression of women's earnings on the sexual composition of 40 occupations showed that, in 1982, for each increase of 10 percent in the proportion of

women in an occupation, median weekly wages fell by \$13 (Mellor 1984).

Most studies of sexual segregation in the labor force have been from status attainment or human capital theoretical perspectives. Accordingly, segregation has been examined in relation to women's educational levels, occupational aspirations, age, and family status. (For a review of relevant studies, see Miller and Garrison [1982]; Reskin and Hartmann [1986].) While these individual-level variables are clearly pertinent to the issue of women's *participation* in the labor force, their ability to explain sexual segregation within the labor force is minimal. For example, Jacobs (1986, p. 204) concludes: "In 1981, assigning to women the age, hours, and educational distributions of men would reduce the degree of segregation by just over 2 percent" (see also England 1982).

We believe that our understanding of occupational sex segregation may be enhanced by a macro-level analysis that focuses on variations among metropolitan areas. The assumption underlying our emphasis on metropolitan areas is that, especially among women, the type of work one does is strongly influenced by several characteristics of the area in which one lives. Specifically, one's job alternatives depend on the mix of jobs available locally, on regional prejudices concerning the job-relevance of one's sex (or race or age), and so on. Women's job placements are especially dependent on characteristics of the local area because of the disinclination of women to relocate. Women are less willing than men to move to another community, 100 or more miles away, even for a much better job (Markham and Pleck 1986). Married women are particularly reluctant to move, and when they do it is most frequently due to their husband's job opportunities; fewer than 20 percent of married women who move

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<sup>1</sup> Further, the inability of most secondary analyses to take into account the sex distribution of occupations within and between firms probably leads to an underestimate of the overall degree of segregation (Bielby and Baron 1986).

have jobs lined up in the new community (Spitze 1986).

Women who work outside the home are particularly dependent on the local job market. Metropolitan areas possess markedly different mixes of occupations, and different occupations are, in turn, associated with highly variable levels of sexual segregation. Thus we expect the job mix in a metropolitan area to be an especially important determinant of occupational sex segregation. In addition, we expect occupational sex segregation to vary with several other characteristics of metropolitan areas, including region, population size, population growth, and economic conditions, as well as the local availability and "competitiveness" of women.

### OCCUPATIONAL STRUCTURE

In their pioneering research, Bowen and Finegan (1969) observed that metropolitan areas differ markedly in their industrial configurations and, accordingly, in the mix of jobs they provide. This mix, they argued, has important implications for the participation of women in the labor force. In order to estimate the *expected* proportion of women in the labor force in any metropolitan area, Bowen and Finegan assumed that the sexual composition of any industry in the metropolitan area would perfectly reflect the sexual composition of the industry nationwide. Thus, for example, metropolitan Gary, Indiana, in 1960 was expected to have a very low ratio of women to men in the labor force because it was dominated by manufacturing industries that nationally employed low proportions of females.

Of course, Bowen and Finegan were comparing industries rather than occupations and they were interested in women's overall labor force participation rather than women's segregation within the labor force. Notwithstanding these differences, their underlying logic is clearly applicable to the problem at hand: we can calculate the expected occupational segregation of the sexes in a metropolitan area by assuming that the sexual composition of each occupation in the area reflects the national composition for the occupation. Thus, a metropolitan area will score high in expected sexual segregation if it is dominated by occupations that nationally are almost exclusively female or almost exclusively male. We interpret this measure of expected sexual segregation as indicating a metropolitan area's structural propensity toward segregating the sexes occupationally.

### OTHER METROPOLITAN AREA VARIABLES

Occupational sex segregation is maintained, in part, by sex role conceptions and imputed

sex-linked abilities, which lead certain jobs to be labeled as "men's work" and others as "women's work" (Oppenheimer 1970; Lipman-Blumen 1984). External barriers often reinforce these labels, sorting people into occupations according to their gender; but segregation is also maintained by sex differences in people's own career choices (Cole 1986). We have identified a number of characteristics of metropolitan areas that may influence the degree to which people use gender as a criterion to sort themselves and others occupationally. These characteristics are discussed in the following paragraphs.

Before introducing these characteristics, we should note that most of them may have a direct impact on a metropolitan area's occupational structure. There are, for example, somewhat distinctive occupational configurations associated with each of the four regions of the United States. However, given the explicit inclusion of occupational structure as a predictor in our model, we will focus on the explanatory power of these other metropolitan area variables net of occupational structure effects.

#### *Region*

In antebellum southern culture, women were regarded as especially ill-equipped to compete with men outside the home (Clinton 1982). Against this backdrop, the participation of southern women in the labor force, while increasing, has continued to lag behind that of women in the rest of the country (Chalmers and Greenwood 1985). In many respects, southerners—and, to a lesser extent, midwesterners—are less inclined generally to be tolerant of innovative roles and ideas than easterners or westerners (Abrahamson and Carter 1986; Corzine, Corzine, and Moore 1986). As a result of these traditional differences, we expected occupational sex segregation to be most pronounced in the south, and least so in the east and west.

#### *Population Size*

People who live in larger cities or metropolitan areas have consistently been shown to be more tolerant of diversity in political, sexual, and other life-style choices (Stephan and McMullin 1982; Abrahamson and Carter 1986). Thus, as changes occur in the range of the jobs women perform, these changes should show up first, and be more evident, in larger metropolitan areas (Fischer 1978). It follows that occupational sex segregation should be inversely related to population size.

#### *Economic-Demographic Vitality*

In metropolitan areas that have experienced recent growth, the production of new jobs may

occur so rapidly that the demand for labor is sufficient to overcome gender-linked stereotypes. On the other hand, in areas beset by declining economic bases, a surplus of people to jobs may reinforce stereotyping, with women either not being able to break into nontraditional jobs or experiencing the familiar "last hired, first fired" phenomenon. Our model includes two indicators of metropolitan area vitality: percent population change, 1970–80, which is expected to have a negative impact on occupational sex segregation; and the unemployment rate as of 1980, which should have a positive effect.

### *Female Labor Force Participation*

The widespread entry of women into the labor force seems to undermine sex segregation based upon the concentration of women into a few (predominantly female) occupations (Snyder, Hayward, and Hudis 1978). We therefore expected that higher female labor force participation rates would be associated with lower levels of occupational sex segregation, especially when a metropolitan area's occupational structure is held constant.

### *Women's Availability and Competitiveness*

The discontinuity of women's careers, primarily as a result of time off for childrearing, appears to have adverse effects on women's mobility and earnings relative to men (Corcoran 1979). By extension, we hypothesize that it may also be directly related to occupational segregation. Because young children are especially likely to keep their mothers away from working outside the home, our indicator of women's availability is the percentage of households in the metropolitan area with children under age six.

Of course, the ability to compete occupationally depends upon level of education. Thus, we also expect occupational sex segregation to vary inversely with women's educational levels. For each metropolitan area, we examined the impact of two educational indicators: women's median years of formal education, and the ratio of women's to men's educational level. Since the two indicators were highly intercorrelated and the former performed slightly better than the latter, the results reported below are for women's median education level.

### UNITS OF ANALYSIS

The population for this study consisted of the 318 Standard Metropolitan Statistical Areas in 1980. Included in this population were all American communities with over 50,000 residents. (Since 1983 these have been termed

*Metropolitan Statistical Areas*, abbreviated as MSAs. We will follow the more recent convention.) Missing data, primarily involving unemployment figures for New England MSAs, reduced the case base to 304. However, our analyses indicate that excluding the 14 cases with missing data does not materially affect our findings.

In our view, MSAs constitute the most appropriate level of geographical aggregation for analyzing employment patterns. Smaller geographical areas, such as central cities or their subunits, are too constricted for our purposes, since they are narrower than the ecological units within which people tend to seek and find employment. Larger areas, such as states, err in the opposite direction, incorporating several different labor markets. "Urbanized areas" are as appropriate as MSAs in many respects, but the use of adjacent county commuting patterns as a criterion in defining MSAs makes them especially meaningful units for analyzing employment patterns.

### MEASUREMENT

Data for all the measures employed here were obtained for 1980 from two census publications: *General Social and Economic Characteristics* (1980, appendix C) and *State and Metropolitan Area Data Book* (1986). Most of these measures, such as the 1980 population, the 1970–80 population growth rate, the percentage of working-age women in the labor force, and so on are self-explanatory, but those pertaining directly to occupational segregation require some explanation.

Our measures of an MSA's structural propensity toward occupational segregation and its degree of occupational segregation involve 42 occupational categories in the 1980 census classification.<sup>2</sup> These are the most detailed and specific occupations for which data are available at the MSA level. In employing occupational rather than industrial categories, we are breaking with the Bowen-Finegan (1969) precedent. However, the heterogeneity of industries as bases for comparison warrants this discontinuation. The basic problem with using industries is that within any industry (e.g., automobile manufacturing), some occupations (e.g., welder) may be dominated by males and others (e.g., secretary) by females. Aggregation to the level of the industry obscures such occupational segregation (Snyder, Hayward, and Hudis 1978). By contrast, the sexual composition of

<sup>2</sup> Because of changes in the census' 1980 occupational classification, it is not directly comparable to earlier classifications (U.S. Department of Commerce 1982; Stevens and Cho 1985).

workforces in a given occupation is less variable from industry to industry. Therefore, focusing on occupational rather than industrial differences in sex segregation should introduce less aggregation bias into our measurement of segregation.<sup>3</sup>

We used census data on the size and sex composition of each occupation in a given metropolitan area to compute the percentage of the MSA's female and male labor forces in each occupational category. These percentages were then used to calculate an Index of Dissimilarity (Duncan and Duncan 1965) as the measure of occupational sex segregation in each MSA. From the large number of available segregation measures, no one of which is correct for all purposes (Lieberman 1980, p. 253), we selected the Index of Dissimilarity (IOD) because of its widespread acceptance and clear interpretability. In the present case, the IOD score is interpretable as the percentage of male workers or female workers in an MSA who would have to change occupations in order to achieve a uniform distribution of the sexes across occupations. Criticism of the IOD has focused largely on the way it treats intercity differences in the minority percentage of the population (e.g., Cortese, Falk, and Cohen 1976). However, this problem is largely alleviated by our focus on male-female rather than black-white differences, since the sexual composition of MSAs is far less variable than the racial composition.

For each MSA we also calculated a measure of the structural propensity toward occupational segregation of the sexes. We began with the *nationwide* female percentage of each occupation and the number of workers in the occupation in a given MSA. Multiplying the former by the latter yielded the number of women who would be expected, on the basis of the occupation's nationwide sexual composition, to be in a certain occupation in a given MSA. Across occupations, these expected numbers and the corresponding expected numbers of males were then used to compute a second IOD score for each MSA, summarizing the extent of sexual segregation expected on the basis of the MSA's occupational structure. This score is our indicator of the structural propensity toward occupational sex segregation. Both IOD

scores are given for all 318 MSAs in the Appendix.

## FINDINGS

Across the 304 metropolitan areas examined here, the mean occupational segregation score, as measured by the IOD for the distribution of men and women in 42 occupational categories, is .513. Thus, on average, 51.3 percent of the employed men or women in an MSA would have to change occupational categories to achieve a uniform occupational distribution for the two sexes. These occupational segregation scores range from a low of .421 in Las Vegas, Nevada, to a high of .635 in Victoria, Texas. In general they are packed in rather tightly around the mean (standard deviation = .035).

We argued earlier that metropolitan areas dominated by occupations that are highly sex-segregated nationally should display unusually high levels of occupational segregation of the sexes. Model 1, summarized in the top section of Table 1, regresses the degree of occupational segregation in an MSA on the MSA's structural propensity toward occupational segregation. Consistent with the notion that occupational segregation is, to a considerable extent, a byproduct of an area's occupational mix, there is a fairly strong relationship ( $r = .54$ ) between the structural propensity toward segregation and the actual degree of segregation. Further, the unstandardized regression coefficient of 0.922 shows that, as the structural propensity toward occupational segregation increases among MSAs, occupational segregation itself increases by almost the same amount. The combined unstandardized regression coefficient and intercept indicate that the amount of occupational segregation in an MSA is very slightly above what would be expected based on the MSA's occupational propensity toward segregation. This is not to say that occupational segregation of the sexes is a simple function of an area's structural propensity toward sex segregation; after all, the lion's share of the variance in occupational segregation remains unexplained after the effects of structural propensity have been taken into consideration.

As further preliminaries to considering the full model, Models 2 and 3 focus on regional differences in the occupational segregation of the sexes. In Model 2, which explores the simple effects of region, we see that, as hypothesized, MSAs in every other part of the country are significantly less sex-segregated occupationally than those in the south (the omitted or reference category). North central and western MSAs are in an intermediate position, and northeastern MSAs tend to be least occupationally sex-segregated. The unstandard-

<sup>3</sup> Although we emphasize an analysis of occupations rather than industries, we do not mean to imply that the latter are unimportant. Industries should be considered on theoretical grounds, in that changes within industries can be important precursors of occupational changes, and on analytical grounds, in that combining industry and occupation provides the most robust predictor of sex differences in income (Treiman and Hartmann 1981).

Table 1. Predicting Occupational Segregation in 304 MSAs

Predictor	Unstandardized Coefficient (b)	Standard Error of b	F-ratio
<i>Model 1</i>			
Structural propensity	.922	.083	124.0***
Intercept	.046		
Multiple R: .539 Adjusted R <sup>2</sup> : .289			
<i>Model 2</i>			
Northeast	-.022	.006	14.2***
North central	-.012	.005	5.7*
West	-.014	.006	5.8*
Intercept	.522		
Multiple R: .229 Adjusted R <sup>2</sup> : .043			
<i>Model 3</i>			
Structural propensity	1.021	.092	123.8***
Northeast	-.003	.005	0.3
North central	.009	.005	4.1*
West	-.012	.005	6.9**
Intercept	-.004		
Multiple R: .574 Adjusted R <sup>2</sup> : .321			
<i>Model 4</i>			
Structural propensity	1.104	.084	172.8***
Northeast	-.003	.005	0.4
North central	.010	.005	3.4
West	.001	.005	0.0
Population size (logged)	-.004	.001	7.2**
Population change	-.061	.014	19.3***
Unemployment rate	.156	.068	5.3*
% families with children	.200	.052	14.7***
% women in labor force	-.126	.033	14.5***
Median female education	-.067	.024	7.6**
Multiple R: .748 Adjusted R <sup>2</sup> : .544			

\*  $p = .05$ .\*\*  $p = .01$ .\*\*\*  $p = .001$ .

ized coefficients, considered in light of the standard deviation of only .035 for occupational sex segregation, indicate some relatively major regional differences. Significant differences of the same magnitude remain when the impact of an area's structural propensity toward segregation is controlled, as in Model 3, but the regional configuration changes. Controlling for structural propensity, the north central region looms as the most occupationally sex-segregated, followed by the south and northeast, which are tightly clustered, and then by the west. Thus the fact that northeastern MSAs tend to be less sex-segregated than MSAs in any other region is attributable to the relative paucity in the northeast of occupations that are highly sex-segregated nationally. Similarly, when structural propensity is taken into consideration, north central MSAs appear to be even more sex-segregated occupationally than their southern counterparts.

Let us now examine the full 10-predictor model. The explanatory power of Model 4 is substantial (multiple  $R = .748$ , adjusted  $R^2 = .544$ ), suggesting that the foremost determinants of occupational sex segregation are in the model. As in Model 3, the structural propensity

toward sexual segregation is dominant, but most of the other variables also display significant independent effects in the expected directions. Further, when combined, the other MSA characteristics account for almost as much explained variation in segregation as the structural propensity measure.

Notable among these other variables are population size and recent population change. Consistent with our hypothesis, larger metropolitan areas are significantly less occupationally sex-segregated. Had we focused on the entire nation rather than solely on metropolitan areas, the impact of population size would presumably have been even greater. For the entire United States as of 1981, Reskin and Hartmann (1986) calculate a segregation score of .62. The differences between our occupational listings and theirs are very slight, and the two data sets are only one year removed from each other. Accordingly, we think it reasonable to conclude that the difference in the two estimates—theirs of .62 and ours of .51—means that there is less occupational sex segregation in metropolitan areas than in smaller, less urban places. Such differences would of course be entirely consis-



tent with the population size hypothesis advanced earlier in this paper.

To an even greater extent than population size, rapid population growth is associated with lower levels of occupational sex segregation. Since the effects of structural propensity have been removed, we can add that larger and faster growing metropolitan areas are significantly less sex-segregated than would be expected on the basis of their structural propensity toward sexual segregation.

Also consistent with our expectations, unemployment emerges as an independent influence on sexual segregation, with MSAs where the unemployment rate is lower having significantly less sex-segregated occupational structures. The three variables indexing the availability and competitiveness of women in the labor force also perform as anticipated: metropolitan areas are less sex-segregated occupationally to the extent that a smaller percentage of families have children; a larger percentage of women are in the labor force; and the average woman has a more extensive educational background.

Finally, one of the most interesting findings in Model 4 is the *noneffect* of region when the other MSA characteristics are taken into consideration. Earlier, in comparing Models 2 and 3, we noted that the imposition of a control for structural propensity lessened the regional patterning of occupational sex segregation by rendering the difference between the northeast and the south nonsignificant and by decreasing somewhat the magnitude of the differences between the south and the north central and west. In other words, Model 3 indicates that the regional differences pinpointed in Model 2 reflect, to a considerable degree, regional differences in occupational structures. When, as in Model 4, other metropolitan characteristics that are themselves regionally differentiated are taken into consideration, no significant regional patterning of occupational segregation of the sexes remains. More than any other variable in the model, population growth is responsible for attenuating the relationship between region and occupational sex segregation. The reason is that the four regions are highly differentiated on the basis of population growth, with mean 1970–80 growth rates of 19.6 percent in the south, 4.2 percent in the northeast, 6.4 percent in the north central, and 27.6 percent in the west ( $F = 73.4$ , 3,300,  $\eta^2 = .643$ ,  $p < .001$ ). Accordingly, controlling for population growth (along with the other predictors in Model 4) in effect pulls the very rapid-growing metropolitan areas of the west back toward the south in terms of occupational sex segregation. Since the effect of region on occupational segregation declines to nonsignificance when a control for population

growth is introduced, there is strong presumptive evidence that differential population growth rates are the keys to the significant regional effects found in Models 2 and 3. More generally, Model 4 strongly suggests that there is nothing unique about the regions themselves that accounts for the extent of occupational segregation in a metropolitan area. Rather, the regional differences in Models 2 and 3 are byproducts of the regional dispersion of factors that lead to segregation, such as population decline.

## CONCLUSIONS

The preceding analysis demonstrates the fruitfulness of analyzing sex segregation from a metropolitan area standpoint. Our findings indicate that the occupational segregation of the sexes varies systematically across metropolitan areas as a function of the structural propensity toward segregation and, net of that propensity, as a function of various metropolitan characteristics. We think a model similar to ours may have the potential to advance our understanding of a wide range of similar phenomena, such as racial discrimination in occupations or industries (Kaufman and Daymont 1981).

This is not to say that we have satisfactorily resolved all the important issues pertaining to differences in occupational sex segregation across metropolitan areas. We view as a significant advance our inclusion of structural propensity as a factor underlying differences among metropolitan areas in occupational sex segregation. However, we also acknowledge that different sexual employment patterns in different occupations—the core of the structural propensity concept—are themselves in need of explanation. Why are some occupations predominantly female and others predominantly male? We have in effect leapfrogged that issue, taking an occupation's national male-female mix as a given, and going on to use that mix, and the mix of occupations within a metropolitan area, as a predictor of sex segregation.

A different type of limitation is that, even though our model performs well, we were not able to include a set of potentially important *organizational* linkages that we believe warrant pursuing. There is some evidence that occupational segregation, sexual differences in internal mobility patterns, and sexual income inequality are related to organizational characteristics, such as firm size (Perrucci 1986; Baron, Davis-Blake and Bielby 1986). These organizational characteristics seem, in turn, related to metropolitan area characteristics, such as population size and growth rate (Lorence 1985; Nord 1984). Thus, to the extent that organizational characteristics, such as firm size, influence

occupational segregation, and to the extent that these same characteristics vary among metropolitan areas, then much could be gained by including organizational characteristics within an expanded version of our explanatory model.

The simultaneous examination of organizational and metropolitan area variables could also help fill some theoretical gaps in the population ecology of organizations literature. Some ecologically driven analyses take note of community features, especially the population size of the metropolitan area (Carroll and Huo 1986). Instead of examining organizations within the context of a surrounding community, however, most population ecology analyses have focused

on the diversity or density of all organizations of a given type. A logical next step is to focus on the interdependence of organizations and other (nonorganizational) metropolitan area characteristics, such as population size, growth rates, educational levels, and so on. It is likely that there are meaningful links among these metropolitan area characteristics and such organizational features as ownership patterns, firm size, and the like. The simultaneous inclusion of variables pertaining to organizations and to metropolitan areas will, we believe, prove fruitful for the study of organizations, the study of metropolitan communities, and the study of many concrete research problems, such as occupational segregation.

Appendix. Actual IOD Score (1) and Structural Propensity (2) for 318 MSAs

MSA	(1)	(2)	MSA	(1)	(2)
Abilene TX	.567	.545	Brownsville TX	.543	.511
Akron, OH	.528	.491	Bryan TX	.534	.510
Albany GA	.494	.507	Buffalo NY	.521	.497
Albany NY	.521	.518	Burlington NC	.516	.469
Albuquerque NM	.485	.516	Burlington VT	.520	.511
Alexandria LA	.593	.551	Canton OH	.528	.484
Allentown PA	.499	.471	Casper WY	.564	.534
Altoona PA	.502	.494	Cedar Rapids IA	.507	.489
Amarillo TX	.540	.531	Champaign IL	.466	.490
Anaheim CA	.485	.489	Charleston SC	.523	.533
Anchorage AS	.500	.525	Charleston WV	.589	.542
Anderson IN	.503	.461	Charlotte NC	.507	.490
Anderson SC	.491	.461	Charlottesville VA	.482	.517
Ann Arbor MI	.426	.478	Chattanooga TN	.507	.490
Anniston AL	.503	.483	Chicago IL	.485	.491
Appleton WI	.491	.477	Chico CA	.533	.519
Asheville NC	.502	.496	Cincinnati OH	.513	.497
Athens GA	.451	.482	Clarksville TN	.523	.513
Atlanta GA	.477	.506	Cleveland OH	.512	.493
Atlantic City NJ	.497	.533	Colorado Springs CO	.468	.508
Augusta GA	.484	.518	Columbia MD	.494	.505
Austin TX	.473	.509	Columbia SC	.486	.512
Bakersfield CA	.591	.534	Columbus GA	.504	.503
Baltimore MD	.496	.513	Columbus OH	.496	.502
Bangor ME	.482	.502	Corpus Christi TX	.578	.543
Baton Rouge LA	.559	.529	Cumberland MD	.520	.524
Battle Creek MI	.500	.489	Dallas TX	.499	.508
Bay City MI	.561	.496	Danbury CT	.518	.478
Beaumont TX	.611	.529	Danville VA	.471	.461
Bellingham WA	.493	.509	Davenport IA	.526	.494
Benton Harbor MI	.495	.484	Dayton OH	.509	.493
Billings MT	.548	.531	Daytona Beach FL	.488	.526
Biloxi MS	.527	.529	Decatur IL	.553	.497
Binghamton NY	.489	.492	Denver CO	.469	.506
Birmingham AL	.558	.521	Des Moines IA	.589	.506
Bismarck ND	.585	.548	Detroit MI	.527	.492
Bloomington IN	.424	.455	Dubuque IA	.541	.500
Bloomington IL	.493	.502	Duluth MN	.543	.529
Boise ID	.518	.518	Eau Claire WI	.491	.506
Boston MA	.484	.505	El Paso TX	.506	.495
Bradenton FL	.513	.533	Elkhart IN	.472	.465
Bremerton WA	.539	.534	Elmira NY	.531	.503
Bridgeport CT	.524	.492	Enid OK	.600	.549
Bristol CT	.496	.470	Erie PA	.506	.477
Brockton MA	.523	.512	Eugene OR	.475	.495

## Appendix Continued

MSA	(1)	(2)	MSA	(1)	(2)
Evansville IN	.516	.505	Las Vegas NV	.421	.530
Fall River MA	.496	.472	Lawrence KS	.435	.477
Fargo ND	.519	.519	Lawrence MA	.483	.479
Fayette NC	.501	.515	Lawton OK	.487	.525
Fayetteville AK	.456	.500	Lewiston ME	.467	.478
Fitchburg MA	.498	.489	Lexington KY	.499	.502
Flint MI	.584	.466	Lima OH	.489	.474
Florence AL	.529	.492	Lincoln NE	.484	.501
Florence SC	.513	.504	Little Rock AK	.512	.523
Ft. Collins CO	.465	.501	Long Branch NJ	.526	.509
Ft. Lauderdale FL	.501	.524	Longview TX	.561	.512
Ft. Myers FL	.528	.544	Lorain OH	.512	.473
Ft. Smith AK	.506	.492	Los Angeles CA	.451	.488
Ft. Walton Beach FL	.527	.537	Louisville KY	.514	.493
Ft. Wayne IN	.507	.491	Lowell MA	.503	.495
Fresno CA	.567	.511	Lubbock TX	.516	.514
Gadsden AL	.508	.492	Lynchburg VA	.500	.497
Gainesville FL	.482	.518	Macon Ga	.513	.521
Galveston TX	.577	.553	Madison WI	.481	.498
Gary IN	.565	.509	Manchester NH	.508	.495
Glens Falls NY	.516	.501	Mansfield OH	.505	.467
Grand Forks ND	.539	.532	McAllen TX	.562	.509
Grand Rapids MI	.480	.482	Medford OR	.514	.512
Great Falls MN	.539	.524	Melbourne FL	.502	.521
Greeley CO	.508	.529	Memphis TN	.510	.511
Green Bay WI	.519	.497	Meriden CT	.522	.472
Greensboro NC	.589	.593	Miami FL	.479	.512
Greenville SC	.487	.466	Midland TX	.571	.538
Hagerstown MD	.501	.497	Milwaukee WI	.502	.484
Hamilton OH	.533	.496	Minneapolis MN	.503	.497
Harrisburg PA	.488	.509	Mobile AL	.552	.522
Hartford CT	.503	.498	Modesto CA	.540	.504
Hickory NC	.474	.447	Monroe LA	.558	.521
Honolulu HA	.473	.510	Montgomery AL	.522	.523
Houston TX	.554	.520	Muncie IN	.521	.481
Huntington WV	.558	.515	Muskegon MI	.523	.480
Huntsville AL	.500	.488	Nashua NH	.488	.475
Indianapolis IN	.508	.498	Nashville TN	.486	.503
Iowa City IA	.460	.506	Nassua NY	.539	.515
Jackson MI	.523	.483	New Bedford MA	.482	.470
Jackson MS	.519	.523	New Britain CT	.498	.470
Jacksonville FL	.528	.530	New Brunswick NJ	.506	.502
Jacksonville NC	.514	.526	New Haven CT	.484	.496
Janesville WI	.480	.481	New London CT	.491	.504
Jersey City NJ	.450	.488	New Orleans LA	.536	.529
Johnson City TN	.492	.477	New York NY	.456	.499
Johnston PA	.556	.523	Newark NJ	.507	.498
Joplin MO	.480	.483	Newark OH	.514	.498
Kalamazoo MI	.477	.489	Newburgh NY	.506	.518
Kankakee IL	.526	.515	Newport News VA	.510	.531
Kansas City MO	.503	.502	Norfolk VA	.516	.535
Kenosha WI	.491	.471	Northeast PA	.502	.480
Killeen TX	.520	.541	Norwalk CT	.498	.467
Knoxville TN	.504	.503	Ocala FL	.532	.529
Kokomo IN	.520	.464	Odessa TX	.584	.527
LaCrosse WI	.509	.509	Oklahoma City OK	.506	.517
Lafayette LA	.582	.539	Olympia WA	.496	.519
Lafayette IN	.456	.477	Omaha NE	.505	.511
Lake Charles LA	.595	.537	Orlando FL	.492	.519
Lakeland FL	.518	.518	Owensboro KY	.537	.502
Lancaster PA	.465	.479	Oxnard CA	.507	.506
Lansing MI	.499	.495	Panama City FL	.548	.527
Laredo TX	.522	.523	Parkersburg WV	.569	.507
Las Cruces NM	.529	.513	Pascagoula MS	.553	.519

## Appendix Continued

MSA	(1)	(2)	MSA	(1)	(2)
Paterson NJ	.488	.484	Sheboygan WI	.478	.463
Pensacola FL	.543	.534	Sherman TX	.521	.501
Peoria IL	.545	.498	Shreveport LA	.521	.529
Petersburg VA	.484	.510	Sioux City IA	.553	.513
Philadelphia PA	.502	.506	Sioux Falls SD	.522	.519
Phoenix AZ	.495	.504	South Bend IN	.493	.486
Pine Bluff AK	.578	.508	Spokane WA	.523	.520
Pittsburgh PA	.547	.510	Springfield IL	.543	.526
Pittsfield MA	.552	.498	Springfield MO	.487	.499
Portland ME	.509	.505	Springfield OH	.516	.491
Portland OR	.486	.498	Springfield MA	.499	.490
Portsmouth NH	.486	.494	Stamford CT	.535	.480
Poughkeepsie NY	.508	.519	State College PA	.441	.476
Providence RI	.467	.480	Steubenville OH	.599	.520
Provo UT	.482	.498	Stockton CA	.545	.520
Pueblo CO	.554	.520	Syracuse NY	.512	.507
Racine WI	.484	.464	Tacoma WA	.513	.517
Raleigh NC	.479	.511	Tallahassee FL	.477	.517
Reading PA	.467	.475	Tampa FL	.504	.522
Redding CA	.536	.538	Terre Haute IN	.490	.498
Reno NV	.437	.523	Texarkana TX	.535	.520
Richland WA	.552	.530	Toledo OH	.535	.504
Richmond VA	.509	.517	Topeka KS	.533	.524
Riverside CA	.518	.523	Trenton NJ	.494	.505
Roanoke VA	.519	.514	Tucson AZ	.482	.524
Rochester MN	.557	.545	Tulsa OK	.539	.513
Rochester NY	.502	.491	Tuscaloosa AL	.533	.527
Rockford IL	.489	.469	Tyler TX	.543	.515
Rock Hill SC	.502	.470	Utica NY	.522	.511
Sacramento CA	.504	.519	Vallejo CA	.537	.534
Saginaw, MI	.560	.485	Victoria TX	.635	.558
St. Cloud MN	.488	.513	Vineland NJ	.504	.501
St. Joseph MO	.509	.505	Visalia CA	.579	.506
St. Louis MO	.521	.510	Waco TX	.484	.501
Salem OR	.514	.512	Washington DC	.452	.512
Salinas CA	.510	.503	Waterbury CT	.519	.483
Salisbury NC	.485	.475	Waterloo IA	.550	.493
Salt Lake City UT	.500	.508	Wausau WI	.489	.491
San Angelo TX	.559	.527	West Palm Beach FL	.507	.530
San Antonio TX	.509	.521	Wheeling WV	.582	.536
San Diego CA	.472	.507	Wichita KS	.506	.507
San Francisco CA	.452	.500	Wichita Falls TX	.557	.527
San Jose CA	.460	.491	Williamsport PA	.460	.478
Santa Barbara CA	.488	.505	Wilmington DE	.530	.514
Santa Cruz CA	.454	.499	Wilmington NC	.498	.507
Santa Rosa CA	.517	.528	Worcester MA	.506	.491
Sarasota FL	.491	.528	Yakima WA	.562	.506
Savannah GA	.550	.533	York PA	.454	.468
Seattle WA	.482	.505	Youngstown OH	.514	.478
Sharon PA	.565	.490	Yuba City CA	.604	.532

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# CROSS-NATIONAL SIMILARITY IN SOCIAL MOBILITY PATTERNS: A DIRECT TEST OF THE FEATHERMAN-JONES-HAUSER HYPOTHESIS\*

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*In 1975, Featherman, Jones, and Hauser formulated the hypothesis that national patterns of circulation mobility are basically the same, while national patterns of observed mobility differ. This hypothesis has often been tested and generally confirmed by means of multiplicative models. Previous tests have been indirect because the relationship of the circulation-mobility pattern to the underlying circulation-mobility frequencies remained unspecified. Using the linear programming approach, we determine circulation-mobility frequencies. Patterns for both observed and circulation mobility are expressed in terms of proportions, rates, and odds ratios. A re-analysis of data for 16 national samples demonstrates that, across countries, the patterns of circulation mobility are less similar than the patterns of observed mobility. An additional analysis for 22 countries shows that odds ratios computed from circulation-mobility frequencies correlate with macrostructural characteristics of societies. The results provide strong evidence against the tested hypothesis.*

Researchers have long been interested in explaining why various characteristics of social mobility differ across countries. In addition to the classical contributions of Lipset and Zetterberg (1956; Lipset and Bendix 1959) and Miller (1960), a succession of articles provides evidence for or against the thesis that social mobility depends on economic development (Matras, 1961; Marsh 1963; Fox and Miller 1965; Cutright 1968; Hazelrigg and Garnier 1976; Tyree et al. 1979; Erikson et al. 1979; McClelland 1980; Grusky and Hauser 1984). Recently, analyzing the general thesis about the relationship between social mobility and economic development, Goldthorpe (1985, p. 549) concluded that such a thesis "can be construed in a number of quite different ways, which call for different kinds of empirical tests." Concurring with this statement, we contribute a comprehensive empirical test of the well-known proposition of Featherman, Jones and Hauser (1975; 1978), named the "FJH hypothesis" by Erikson et al. (1982).

## ANALYSIS OF THE FJH HYPOTHESIS AND ITS INDIRECT TESTS

The original formulation of the FJH hypothesis states "industrial societies can be shown not to have the same rates of *observed* mobility. However, there is reason to suppose they may have similar patterns of *circulation* mobility" (Featherman et al. 1978, p. 88-9). Further, "... the genotypical pattern of mobility (circulation mobility) in industrial societies with a market economy and a nuclear family system is basically the same. The phenotypical pattern of mobility (observed mobility) differs . . ." (Featherman et al. 1978, p. 89). More recently, Erikson et al. (1982, p. 2) restated this hypothesis: "The variation actually observed in mobility rates of advanced industrial societies . . . is essentially of a structurally induced kind, and . . . a basic similarity may thus prevail in the 'regimes' of exchange mobility. . . ." Kerckhoff et al. (1985, p. 282) wrote that, according to the FJH hypothesis, "the similarity among industrialized societies would be in circulation mobility and not in total mobility, which includes both circulation mobility and structural mobility."

Although these quotations present the essence of the hypothesis, its meaning is imputed by the theoretical and methodological context in which it appears. Various researchers have subsequently used a new terminology that changes the meaning of the original formulation. Some departures involve searching for invariance in social fluidity instead of attempting to compare the similarity of circulation-mobility patterns

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with the similarity of total-mobility patterns. We restrict our analysis to the original formulation and to those restatements that are formally equivalent to it. In this section we discuss some concepts involved in the hypothesis, theoretical arguments for and against it, and the indirectness of its previous tests.

### *The Concept of Circulation Mobility*

Featherman, Jones, and Hauser saw their hypothesis as a revision of Lipset and Zetterberg's (1959) proposition that, among industrial societies, the proportion of persons not inheriting their father's status is very similar. The revision restricts the proposition to a *particular kind of mobility*, called *circulation mobility*, or *exchange mobility*. Since any kind of occupational mobility denotes movements from origins to destinations, circulation mobility must be either defined or expressed in terms of these movements.

The fundamental question is *what constitutes circulation mobility*, understood as "*exchanges between occupations?*" (Grusky and Hauser 1984, p. 19). Although the authors of the FJH hypothesis do not explicitly define circulation mobility, one can assume that they use the commonly accepted social-scientific notion of exchange. The notion is used in sociology (e.g., referring to relationships in social networks), anthropology (e.g., with respect to gift giving), and economics (e.g., in studying monetary flows and market transactions). As used in everyday language and technical terminology, exchange refers to an "[a]ct of giving . . . one thing in return for another as an equivalent" (Webster's New Collegiate Dictionary 1956 p. 287). In the case of occupational mobility, exchange means that each occupational category "gives" to all other categories a certain number of persons "in return for . . . an equivalent." The concept of circulation or exchange mobility implies that the distributions of origin and destination are the same. This is an accepted understanding of circulation mobility and no departure from it was mentioned in the original elaboration on the hypothesis.

The authors of the FJH hypothesis made clear that its testing requires ". . . an analytical ability to distinguish circulation from structural mobility" (Featherman et al. 1978, p. 90). This analytical ability should result in displaying *circulation and structural mobility as components of total mobility*. The authors of the FJH hypothesis imply they understand both kinds of mobility as components of total mobility in statements such as "Once structural mobility has been taken into account [in total mobility], circulation mobility has been nearly constant over time" (Featherman et al. 1978, p. 89). In

testing the hypothesis, later investigators expressed this idea in a similar way, e.g., "total mobility, which includes both circulation mobility and structural mobility" (Kerckhoff 1985, p. 282). Accordingly, the frequencies of transitions of either circulation mobility or structural mobility should not exceed the corresponding frequencies of gross mobility since a part cannot be larger than the whole.

Simultaneously with the development and application of multiplicative models to the analysis of mobility tables, the meaning of circulation mobility has changed. This change is understandable since attempts to find the frequencies of circulation mobility were long unsuccessful; either some of the frequencies obtained were negative or exceeded the frequencies of total mobility, or circulation mobility was restricted to reciprocal flows. The intractability of the problem has led some researchers to equate certain characteristics of statistical association with circulation mobility. In consequence, the FJH hypothesis became understood as a statement about fluidity in observed mobility instead of being concerned with patterns of circulation mobility. In this paper we refer to the original formulation of the FJH hypothesis and use the frequencies of circulation mobility to determine its patterns.

### *Theoretical Arguments*

The FJH hypothesis implies that a certain level of economic development unifies cross-national patterns of circulation mobility, while patterns of observed mobility remain diversified. The hypothesis has not been given a systematic theoretical justification which would specify the mechanism producing these effects. Featherman et al. (1978, p. 89) mentioned only that the pattern of observed mobility "differs according to the rate of change in the occupational structure, exogeneously determined . . . by . . . technological change, the supply and demand for specific kinds of labor . . . and changing social values affecting . . . the demand for higher education, the rate of economic change, family size, and the spacing of children." Recently, Grusky and Hauser (1984, p. 35) provided clues for explaining the invariance of patterns of circulation mobility by positing that it "may be the analogue to invariance in prestige hierarchies, in the sense that both may result from cross-national regularities in the resources and desirability accorded occupations."

Industrialization creates similar occupational structures and forces similar flows between occupational origins and destinations. According to the convergence theory of economic development (Inkeles 1981; see Form 1979 for a review of the literature), countries (become

similar in those aspects necessitated by a path of development stemming from the "logic of industrialization." The uniform direction of change in the occupational structure is certainly one of these aspects. Therefore one can expect that industrialization would produce similar patterns in the structural component of mobility, resulting in a uniform pattern of *total* mobility.

Why should the pattern of circulation mobility be invariant with respect to economic development? According to some theoretical arguments, the rates of circulation mobility are not impervious to such factors as the level of education, the distribution of mass communication, the level of urbanization, and the rate of geographical mobility (Treiman 1970). Moreover, social values rooted in the history of each country affect patterns of circulation mobility independently of economic development. Still, such countries as the United States, France, and Japan differ with respect to popular standards of success and emphasis on individual achievement. We think that it would be more difficult for a sociologist to explain *a priori* why cross-national differences do not affect a "free," nonstructural part of mobility than to explain some of its variation.

#### *Indirectness of Tests*

Recent tests of the hypothesis used data from pairs or triples of countries (Erikson et al. 1982; McRoberts and Selbee 1981; Breen 1985) and from larger sets (McClendon 1980; Grusky and Hauser 1984). All major tests focusing on the FJH hypothesis were based on the multiplicative approach, which typically involves (a) specification of the model(s) that represent the pattern of circulation mobility; (b) estimation of the parameters of this model for a given data set; and (c) evaluation of the goodness of data-fit. If there is a good data-fit to a common cross-country model, the hypothesis is supported.

Researchers using this strategy do not investigate the relationship between the circulation-mobility pattern and circulation-mobility frequencies. Surprisingly, some basic questions are ignored. Do the discovered patterns of circulation mobility—or "patterns of fluidity" or "patterns of openness"—correspond to exchanges between origin and destination categories? What are the magnitudes of these exchanges? Because the relationship between circulation mobility (an object) and its pattern (an object's property) is not clarified in the multiplicative approach, we treat previous tests of the FJH hypothesis as indirect. One of the constituent objects of the FJH hypothesis, circulation mobility, *remains outside the scope of the investigation.*

Frequencies implied by the proposed patterns of circulation mobility cannot be represented in

terms of exchanges (since they do not result in identical distributions of origins and destinations), and they are not components of total mobility (since they exceed corresponding elements of observed mobility). For a discussion of these issues, see Krauze and Slomczynski (1986, p. 264–5.) Parameters of multiplicative models measure certain patterns of association in the mobility table rather than distinguish between particular kinds of mobility transition. Since the FJH hypothesis refers to the patterns of circulation mobility, any test using the multiplicative-modeling approach would have to adequately represent the frequencies of circulation mobility.

#### DESIGNING A DIRECT TEST OF THE FJH HYPOTHESIS

In its original formulation, the FJH hypothesis calls for a comparison of the intercountry similarity of observed-mobility patterns with the intercountry similarity of circulation-mobility patterns. The direct test consists of comparing the same characteristics of both kinds of mobility patterns. We assume that elements from which "patterns" of mobility are constructed include mobility proportions, rates, and odds ratios. Thus, the testable implication of the FJH hypothesis, in its generalized version considered by Grusky and Hauser (1983), is: *Among countries, national patterns of observed mobility are less similar than national patterns of circulation mobility.*

Direct testing of the FJH hypothesis requires that: (1) comparable mobility data for a set of countries are available; (2) the frequencies of both observed and circulation mobility are computed on the basis of mobility data; (3) the "mobility pattern" is operationally defined in the same way for both observed and circulation mobility; (4) a measure of the similarity of the patterns is established; (5) the criteria for rejection of the hypothesis are specified.

#### *Mobility Data for Selected Countries*

The FJH hypothesis is restricted to countries as units of analysis; ideally, scholars should have mobility data from a representative sample of *all* countries. In practice, cross-national studies of intergenerational social mobility are limited to "accidental" samples of countries for which data are available. As Hazelrigg and Garnier (1976, p. 500) point out, the minimal requirement for such samples is that the countries have different levels of economic and political development. Data matrices must have identical occupational categories across countries. Since Grusky and Hauser's (1983) data for 16 countries satisfy the requirement of comparability and have been used for testing the FJH



Table 1. Definitions of Three Mobility Patterns for the  $k$ th Order Matrix  $X = (x_{ij})$  of Mobility Frequencies

Type of Pattern	Definition of Elements of Pattern	Notation for Pattern Matrix	Abbreviated Notation
Pattern of proportions	$\alpha_{ij} = x_{ij}/x_{..}$ , $i, j = 1, \dots, k$	$A = (\alpha_{ij})$ , $i, j = 1, \dots, k$	$A = A(X)$
Pattern of inflow and outflow rates	$\beta_{ij} = x_{ij}/x_{i.}$ $\beta_{k+i, k+j} = x_{ij}/x_{.j}$ $\beta_{i, k+j} = \beta_{k+i, j} = 0$ , $i, j = 1, \dots, k$	$B = (\beta_{uv})$ , $u, v = 1, \dots, 2k$	$B = B(X)$
Pattern of odds ratios	$\gamma_{ijuv} = (x_{ij}x_{uv})/(x_{uj}x_{iv})$ , $i < u \leq k, j < v \leq k$	$G = (\gamma_{wz})$ , $w, z = 1, \dots, k(k-1)/2$	$G = G(X)$

hypothesis, we reanalyze these data. These mobility tables include the distinction between white-collar, blue-collar and farm workers.

#### Frequencies of Observed and Circulation Mobility

We assume that the frequencies of observed (total) mobility for each country available from a cross-tabulation of raw data are subject only to sampling error. Circulation mobility, as a particular kind of mobility, should also be expressed in terms of frequencies of transitions between each pair of origin and destination categories. Following Krauze and Slomczynski (1986), we define circulation mobility as (1) the part of total mobility (2) consisting of interchange status transitions, (3) which result in identical origin and destination distributions; it is (4) limited to interchange status transitions and exhausts them. Interchange status transitions are direct and indirect exchanges among status categories. These transitions were shown by Krauze and Slomczynski (1986) to be decomposable into cycles capturing the essence of exchange; we use this definition because it expresses the underlying notion of circulation.

The frequencies of circulation mobility can be computed by means of linear programming. The linear program is: For a given matrix of observed mobility  $N = (n_{ij})$ , find the matrix  $C = (c_{ij})$  such that

- $\sum_{i,j} c_{ij}$  is maximized under constraints:
- $0 \leq c_{ij} \leq n_{ij}$
- $\sum_j c_{ij} = \sum_j c_{ji}$  for all  $i$ .

In this program, the definitional requirement (1) corresponds to (b), (3) corresponds to (c), and (4) to (a). It is proven that the definitional requirement (2) is satisfied since  $C$  is decomposable into cycles with equal row and column margins. For the three-by-three matrices, the mobility frequencies of circulation mobility  $c_{ij}$  are uniquely determined.

We notice that for a given matrix  $N$  stayers are included in the corresponding matrix  $C$ . This is important for our testing procedure since the

FJH hypothesis is not restricted to mobiles in either  $N$  or  $C$ . Moreover, stayers were included in some previous tests utilizing the concept of fluidity (e.g., Erikson et al. 1982).

#### Patterns of Mobility

We define the pattern of mobility as a transformation of the matrix of frequencies that allows one to retrieve these frequencies up to a scaling factor. According to this formal definition, the matrix of raw frequencies forms a pattern with the scaling factor equal to one. However, for comparative purposes, this pattern is of limited value since it depends on the sample size. In formulating and testing the FJH hypothesis, three kinds of mobility patterns have been considered and are analyzed in this paper: mobility proportions, inflow/outflow rates, and odds ratios (supplemented by some crossing odds). Table 1 formally defines these quantities, using the well-known summation convention.

Although neither the matrix of all outflow rates nor the matrix of all inflow rates forms a pattern by itself, together they do. The pattern of rates is a square matrix of order  $2k$ , where  $k$  is the number of countries. We assume that the row sums and column sums are positive numbers. The pattern of all possible different odds ratios is a square matrix of order  $k(k-1)/2$ . If an element in the denominator is zero, the odds ratio is set equal to zero. For practical purposes, it is sufficient to take into account only nonredundant odds ratios (Bishop et al. 1975); they reproduce the initial matrix of frequencies up to a scalar constant only if they are supplemented by crossing odds; both quantities considered together form a pattern. In our analysis four crossing odds are taken into account:  $x_{11}/x_{21}$ ,  $x_{11}/x_{12}$ ,  $x_{12}/x_{13}$ , and  $x_{21}/x_{31}$ .

We assume that the same elements, proportions, rates, and odds ratios, would be used in forming patterns of both observed and circulation mobility. Ultimately, mobility patterns, as configurations of matrix elements, are derived from observed mobility frequencies (for the observed mobility pattern) or from circulation mobility frequencies (for the circulation mobility pattern). The patterns of observed mobility are denoted  $A(N)$ ,  $B(N)$ ,  $G(N)$  for proportions,

rates, and odds ratios. The corresponding patterns for circulation mobility are denoted  $A(C)$ ,  $B(C)$ , and  $G(C)$ .

### Similarity of Patterns

Mobility patterns, regardless of the nature of their elements—proportions, rates, or odds ratios—are systemic properties of sampled countries. The measurement of similarity of a given kind of mobility pattern for two countries should be based on the closeness of corresponding elements in the patterns, averaged over all elements of the pattern. Since mobility patterns are expressed as matrices, their similarity can be assessed by a distance function defined in the metric space. The points of this space are matrices of patterns of a given kind; the smaller the distance, the greater the similarity between the compared patterns.

Let  $\rho_1$  be the distance function defined on patterns of proportions. For any pair of matrices of mobility frequencies  $X^1$ ,  $X^2$ , the Euclidean distance between them is given by

$$\rho_1 \{A(X^1), A(X^2)\} = \left\{ \sum_{i,j=1}^k (\alpha^1_{ij} - \alpha^2_{ij})^2 \right\}^{1/2}$$

Where  $A(X^1) = (\alpha^1_{ij})$  and  $A(X^2) = (\alpha^2_{ij})$ . In a similar way, we define the distance functions  $\rho_2$ —(for patterns of rates) and  $\rho_3$ —(for odds ratios). We utilize the Euclidean distance function, selected from possible functions satisfying the distance axioms, because of its widespread use in measuring similarity in applications of taxonomic methods (Sokal and Sneath 1963).

### Criteria of Rejection

The FJH hypothesis can be formally expressed for each of the three types of mobility patterns and a set of countries  $1, \dots, w$ . The formulation we plan to test for the proportions pattern and every pair of countries  $r, s$  is

$$\rho_1 \{A(C^r), A(C^s)\} < \rho_1 \{A(N^r), A(N^s)\} \quad (1)$$

$r < s$

Let us check this formulation against a testable implication of the FJH hypothesis, which, for the proportions pattern, reads: among countries, national patterns of observed-mobility proportions are less similar than national patterns of circulation-mobility proportions. The left side of inequality (1) involves circulation mobility matrices  $C^r$  and  $C^s$  for countries  $r$  and  $s$  and their derived patterns of proportions  $A(C^r)$  and  $A(C^s)$ . The similarity of these patterns is assessed by means of the distance function  $\rho_1$ .

The same function appears on both sides of inequality (1), since similarity should be assessed in the same way for patterns of circulation and total mobility. On the right-hand side of the inequality, we have matrices of proportions of total mobility,  $A(N^r)$  and  $A(N^s)$ , for the same countries. The direction of inequality is consistent with the testable implication of the FJH hypothesis for two countries,  $r$  and  $s$ .

The operationalization of the testable implication of the FJH hypothesis for the pattern of rates and the pattern of odds ratios is

$$\rho_2 \{B(C^r), B(C^s)\} < \rho_2 \{B(N^r), B(N^s)\} \quad (2)$$

$r < s$

$$\rho_3 \{G(C^r), G(C^s)\} < \rho_3 \{G(N^r), G(N^s)\} \quad (3)$$

$r < s$

We can check formulations (2) and (3) in an analogous manner. The hypothesis is separately tested for all pairs of countries  $r, s$  where  $r < s \leq w$ . Each of the  $w(w-1)/2$  pairwise comparisons gives a decisive result, either supporting or not supporting the hypothesis.

Grusky and Hauser (1984, p. 19) noted that "there is an element of subjectivity in any evaluation of the FJH revision; it is unclear how much similarity in mobility regimes is necessary to confirm the hypothesis." However, this observation does not preclude formulating clear criteria for rejecting the hypothesis according to our testing procedure. A very liberal criterion for rejecting the hypothesis is that, in a substantial number of pairwise intercountry comparisons, (for example, one third of all cases), the direction of inequalities (1), (2), or (3) is inconsistent with the hypothesis.

### REANALYSIS OF DATA FROM SIXTEEN COUNTRIES AND THE TEST OF THE FJH HYPOTHESIS

This section has two purposes: first, to present new analyses of exchange in circulation mobility and, second, to describe the results of our direct test of the FJH hypothesis. The empirical basis of the section is the set of mobility tables provided by Grusky and Hauser (1983) and previously used by Hazelrigg and Garnier (1976) and McClendon (1980).

#### Stayers and Movers in Circulation Mobility

According to our definition, circulation mobility consists of stayers and all movers who participate in exchanges among occupational categories. Table 2 shows the decomposition of observed (total) mobility into frequencies of immobility, remaining circulation mobility, and

Table 2. Decomposition of Total Mobility Matrices into Three Components: Immobility (Bold face), Remaining Circulation Mobility (Roman print), and Structural Mobility (Italics), for 16 Countries with Sample Sizes Standardized to 1,000

Father's Occupation	Son's Occupation			Son's Occupation		
	White Collar	Blue Collar	Farm	White Collar	Blue Collar	Farm
Australia, 1965						
White-collar	<b>158</b>	92	16	<b>296</b>	59	7
	—	0	0	—	0	0
Blue-collar	108	<b>328</b>	20	66	<b>258</b>	4
	48	—	0	112	—	0
Farm	0	36	<b>94</b>	0	11	<b>77</b>
	44	56	—	61	49	—
Belgium, 1968						
White-collar	<b>174</b>	75	4	<b>96</b>	72	5
	—	0	0	—	0	0
Blue-collar	79	<b>262</b>	18	59	<b>285</b>	25
	42	—	0	0	—	0
Farm	0	22	<b>185</b>	18	12	<b>196</b>
	55	84	—	81	151	—
Denmark, 1972						
White-collar	<b>174</b>	75	4	<b>96</b>	72	5
	—	0	0	—	0	0
Blue-collar	79	<b>262</b>	18	59	<b>285</b>	25
	42	—	0	0	—	0
Farm	0	22	<b>185</b>	18	12	<b>196</b>
	55	84	—	81	151	—
Finland, 1972						
White-collar	<b>174</b>	75	4	<b>96</b>	72	5
	—	0	0	—	0	0
Blue-collar	79	<b>262</b>	18	59	<b>285</b>	25
	42	—	0	0	—	0
Farm	0	22	<b>185</b>	18	12	<b>196</b>
	55	84	—	81	151	—
France, 1964						
White-collar	<b>209</b>	100	7	<b>40</b>	16	1
	—	4	0	—	0	0
Blue-collar	94	<b>237</b>	6	17	<b>217</b>	29
	0	—	0	68	—	0
Farm	13	0	<b>159</b>	0	30	<b>311</b>
	46	125	—	43	228	—
Hungary, 1963						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
Italy, 1963						
White-collar	<b>209</b>	67	12	<b>49</b>	23	16
	—	0	0	—	0	0
Blue-collar	79	<b>206</b>	26	19	<b>60</b>	23
	88	—	0	0	—	0
Farm	0	38	<b>109</b>	20	19	<b>637</b>
	95	109	—	45	89	—
Japan, 1965						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
Norway, 1972						
White-collar	<b>209</b>	67	12	<b>49</b>	23	16
	—	0	0	—	0	0
Blue-collar	79	<b>206</b>	26	19	<b>60</b>	23
	88	—	0	0	—	0
Farm	0	38	<b>109</b>	20	19	<b>637</b>
	95	109	—	45	89	—
Philippines, 1968						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
Spain, 1968						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
Sweden, 1972						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
Switzerland, 1969						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
United States, 1962						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
West Germany, 1969						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
Yugoslavia, 1962						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—
West Malaysia, 1966						
White-collar	<b>174</b>	56	8	<b>249</b>	65	11
	—	0	0	—	0	0
Blue-collar	64	<b>217</b>	17	76	<b>138</b>	11
	14	—	0	9	—	0
Farm	0	25	<b>239</b>	0	22	<b>125</b>
	53	133	—	152	142	—

structural mobility. Inspection of this table reveals that, among circularly mobiles, symmetric exchanges leave a residual that needs to be explained. Specifically, with the exception of Sweden, in all countries some exchange, in the form of a cycle involving three occupational categories simultaneously occurs. One such cycle is: from white-collar to farm, from farm to blue-collar, and from blue-collar to white-collar. The other cycle has the opposite direction: from farm to white-collar, from white-collar to blue-collar, and from blue-collar to farm.

To illustrate the difference between the two cycles, Figure 1 presents two matrices of circulation mobility from which the symmetric flows were removed. Each matrix is accompanied by a diagram, graphically showing the circularity and directionality of exchange. Removing all symmetric exchanges makes it easier to notice that the Norwegian matrix contains more upward than downward mobility; for the Finnish matrix the opposite is true. *Upward mobility cycle and downward mobility cycle* are two forms of nonsymmetric exchange in the three-by-three matrix.

In Finland, a certain amount of upward circulation mobility, from farm to white-collar, is compensated for by the twice-larger downward mobility of some persons with blue-collar origin who become farmers and some persons with white-collar origin who become blue-collar workers. In consequence, fewer persons experi-

ence upward than downward mobility, even though the distribution of origin is equal to the distribution of destination. Of course, adding the symmetric exchanges cannot change this result, since in all symmetric exchanges the sum of upward movements is equal to the sum of downward movements.

With the exception of Sweden, the one-way flow from white-collar to farm occurs in all countries (see Table 2). However, in 10 countries this flow is not accompanied by a symmetric flow from farm to white-collar. As shown in Table 3, in Australia, Belgium, Denmark, Hungary, Italy, Japan, Norway, Spain, the United States, and Yugoslavia, the absence of symmetric exchanges between white-collar and farm occurs jointly with the upward-mobility cycle. Only in West Germany does the same cycle appear with symmetric exchanges between all pairs of occupational categories, white-collar and farm included. In Finland, the Philippines, and West Malaysia, symmetric exchanges are accompanied by a downward-mobility cycle, that is, a flow from farm to white-collar, from white-collar to blue-collar and from blue-collar to white-collar. France seems to be an exception, where the downward mobility cycle is forced by an absence of circular flow from farm to blue-collar.

This qualitative description should be supplemented by an analysis of frequencies of nonsymmetric exchanges. In five countries

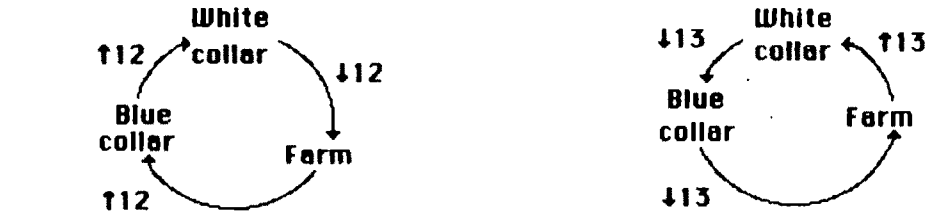
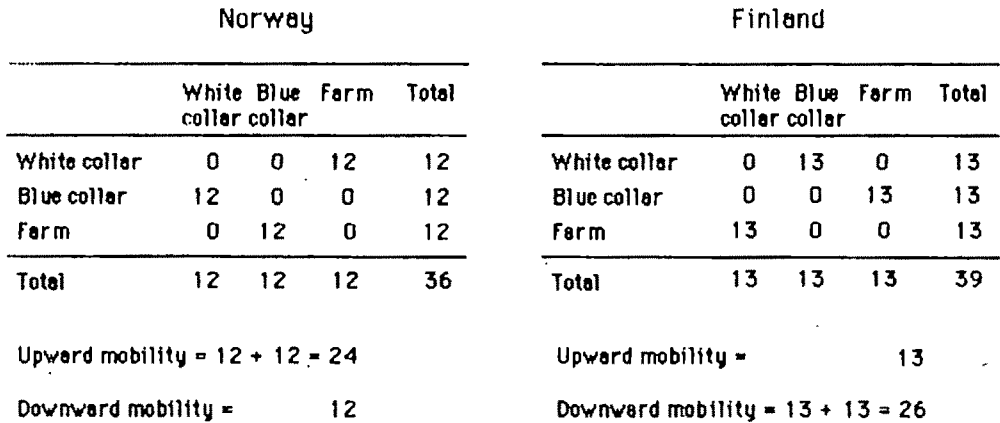


Fig. 1. Direction of Nonreciprocal Circulation Mobility

Table 3. Qualitative and Quantitative Characteristics of Circulation Mobility for 16 Countries

Country and Year	Missing Symmetric Exchange in Circulation Mobility <sup>a</sup>	Three-element Cycle: Upward (U), Downward (D) <sup>b</sup>	Stayers as Proportion of Total Sample	Circularly Mobiles as Proportion of Total Sample	Nonsymmetric Flows as Proportion of the Amount of Circulation Mobility
Australia, 1965	W-F	U	.580	.272	.176
Belgium, 1968	W-F	U	.631	.147	.143
Denmark, 1972	W-F	U	.621	.198	.061
Finland, 1972	None	D	.577	.191	.204
France, 1964	B-F	D	.605	.220	.082
Hungary, 1963	W-F	U	.568	.093	.032
Italy, 1963	W-F	U	.630	.170	.141
Japan, 1965	W-F	U	.512	.185	.178
Norway, 1972	W-F	U	.524	.222	.162
Philippines, 1968	None	D	.746	.120	.100
Spain, 1968	W-F	U	.619	.129	.163
Sweden, 1972	W-F	None	.579	.148	.000
United States, 1962	W-F	U	.514	.158	.076
West Germany, 1969	None	U	.601	.249	.181
West Malaysia, 1966	None	D	.654	.225	.173
Yugoslavia, 1962	W-F	U	.552	.138	.217

<sup>a</sup> W-F: exchange between white-collar and farm; B-F: exchange between blue-collar and farm.

<sup>b</sup> U: from white-collar to farm, from farm to blue-collar, and from blue-collar to white-collar; D: from farm to white-collar, from white-collar to blue-collar, and from blue-collar to farm.

(Denmark, France, Hungary, Sweden, and the United States), persons who are involved in these exchanges constitute less than 10 percent of all circularly mobiles. In all other countries, this percentage is large enough to warrant that nonsymmetric exchanges are an empirically important part of circulation mobility. Thus, the approach of Sobel et al. (1985), which identifies circulation mobility with symmetric exchanges has only a limited value for cross-national studies.

Since our algorithm for computing the frequencies of circulation mobility exhausts all exchanges, the remaining part of total mobility is structural. In all 16 countries, there is the expected upward shift in occupational distribution: the proportion of white-collar workers increases while the proportion of farmers decreases between generations. The most common feature of the mobility caused by this shift is a transition of persons from farm to white-collar and blue-collar categories. Usually, however, structural mobility is not limited to this flow but also includes a flow from the blue-collar to the white-collar category. (cf. Table 2).

For 16 countries, the proportion of circularly and structural mobiles varies from .254 to .488. If the Philippines and West Malaysia, the least industrialized countries, are excluded from the sample, the lower bound of the range rises to .370. The proportion of circularly mobiles varies from .090 to .249. Lipset and Zetterberg (1959, p. 13) stated that in industrialized

countries the proportion of mobile persons appears to be much the same. Indeed, the maximum intercountry difference between proportions is .118 among 14 countries; this seems surprisingly small. For the same 14 countries, the range of the proportion of circularly mobiles is higher, .179, with the lower bound of .093 (for Hungary) and upper bound of .272 (for Australia). A comparison of range statistics does not suggest that the patterns of circulation mobility would be more similar than the patterns of observed (total) mobility.

#### Results of the Test

Table 4 contains the numerical results of the analysis of proportions patterns. According to our operationalization of the FJH hypothesis in formula (1), the hypothesis is confirmed for a pair of countries if the entry above the diagonal is larger than its symmetric entry below the diagonal. A count of symmetric entries shows that the FJH hypothesis for proportions patterns is rejected in 105 out of the 120 cases. The cases in which the hypothesis is not rejected do not form any compact cluster of countries.

Table 5 is more condensed than Table 4. It presents the results of comparisons of symmetric entries in matrices of distances for rates patterns and odds ratios patterns. The entry above the diagonal is equal to one if formula (2) is satisfied for both outflow rates and inflow rates; otherwise it is zero. Similarly, the entry below the diagonal is equal to one if formula (3) is

Table 4. Intercountry Distances in the Pattern of Proportions for Total Mobility (Above Diagonal) and Circulation Mobility (Below Diagonal) for 16 Countries

	AU	BE	DE	FI	FR	HU	IT	JA	NO	PH	SP	SW	US	WG	WM	YU
AU	—	.17	.12	.18	.14	.33	.21	.27	.15	.63	.28	.07	.11	.26	.49	.36
BE	.21	—	.18	.29	.17	.42	.25	.23	.12	.67	.31	.12	.17	.18	.52	.40
DE	.14	.21	—	.13	.07	.25	.10	.18	.12	.52	.16	.15	.14	.19	.38	.26
FI	.17	.30	.11	—	.15	.19	.12	.22	.20	.51	.16	.22	.19	.26	.37	.21
FR	.15	.17	.08	.17	—	.28	.11	.15	.11	.55	.18	.16	.14	.14	.40	.27
HU	.41	.50	.31	.25	.37	—	.19	.30	.32	.40	.16	.36	.31	.35	.28	.13
IT	.23	.27	.09	.15	.13	.25	—	.16	.17	.45	.08	.23	.21	.18	.31	.17
JA	.28	.21	.18	.28	.15	.40	.16	—	.15	.52	.17	.26	.22	.10	.37	.23
NO	.15	.13	.12	.22	.09	.41	.17	.14	—	.59	.22	.13	.11	.15	.44	.30
PH	.73	.77	.60	.58	.63	.37	.51	.59	.67	—	.39	.67	.64	.55	.16	.35
SP	.33	.36	.19	.21	.23	.20	.10	.21	.27	.42	—	.30	.27	.21	.24	.10
SW	.12	.15	.19	.23	.19	.46	.27	.30	.18	.78	.37	—	.08	.25	.52	.38
US	.08	.15	.16	.21	.16	.44	.25	.28	.15	.76	.35	.03	—	.24	.50	.34
WG	.29	.20	.22	.32	.17	.45	.21	.06	.14	.64	.27	.30	.29	—	.40	.30
WM	.55	.60	.43	.42	.46	.27	.35	.42	.50	.19	.25	.61	.59	.46	—	.21
YU	.42	.47	.29	.29	.34	.16	.21	.32	.37	.31	.12	.48	.46	.37	.15	—

Note: AU—Australia, BE—Belgium, DE—Denmark, FI—Finland, FR—France, HU—Hungary, IT—Italy, JA—Japan, NO—Norway, PH—Philippines, SP—Spain, SW—Sweden, US—United States, WG—West Germany, WM—West Malaysia, YU—Yugoslavia.

satisfied for both odds ratios and supplementary odds; otherwise it is zero. A count of ones shows that the FJH hypothesis is rejected in 65 cases for rates patterns and in 108 cases for odds ratios patterns.<sup>1</sup>

Pairwise comparisons of distances between matrices involve their functional dependence. For example, for 120 distances between matrices, a set of 92 distances determines all others. However, the inequality between the distance for a pair of circulation-mobility matrices and the distance for the corresponding pair of total-mobility matrices is not dependent in the functional sense but only in the stochastic one. To decrease the stochastic dependence, one can construct a subsample of, for example, three-fourths of the total cases. A conservative statistical test, favoring the nonrejection of the FJH hypothesis, is based on two assumptions: first, that all inequalities confirming the hypothesis occur in the "dependence-free" subsample, and second, that these inequalities constitute a substantial majority of all cases, e.g. at least 66 percent. For not rejecting the hypothesis at .05 significance level, the number of inequalities confirming the hypothesis would have to be at

least 68 for the subsample of 90 cases. This number is larger than the numbers 15, 55, and 12 obtained in the three described tests. Thus, the FJH hypothesis is not supported on statistical grounds.

#### NEW ANALYSIS: FLUIDITY IN CIRCULATION MOBILITY

The analysis presented in this section, based on data collected in 22 countries between 1962 and 1978, explores cross-country variation in circulation-mobility patterns. In the set of data provided by Grusky and Hauser (1983) we substituted some tables with more recent and/or more reliable data for eight countries (Australia, France, Hungary, Italy, Japan, Sweden, the United States, and West Germany). We also added six countries to the set (Austria, Canada, Czechoslovakia, England and Wales, New Zealand, and Poland).<sup>2</sup>

For each of the 22 countries, a matrix of circulation mobility was determined and its internal association between origins and destina-

<sup>1</sup> If for outflow and inflow rates the common distance is considered, formula (2) is satisfied in 89 cases, generally by a small margin. The statistical test for difference among averaged distances over matrix cells shows that at an acceptable level of significance  $p < .05$  the FJH hypothesis is not supported in more than 60 percent of the cases. Odds ratios and supplementary odds differ in their magnitude to such an extent that considering them in space (by one distance) could produce distorted results. Formula (3) is satisfied in 14 cases for odds ratios and in 56 cases for the set of supplementary odds.

<sup>2</sup> The data for Belgium, Denmark, Finland, Norway, the Philippines, Spain, West Malaysia, and Yugoslavia are from Grusky and Hauser (1983). The sources of data for other countries are: for England and Wales, France, and Sweden—Erikson et al. (1979); for Australia—Broom et al. (1980); for Austria—Haller and Mach (1984); for Canada—Goyder and Curtis (1977); for Czechoslovakia—Československý Vědecký Ústav Práce (1972); for Hungary—Andorka (1976); for Italy—Ammassari (1977); for Japan—Kiso Shu Kei Ito (1976); for New Zealand—Davis (1979); for Poland—Zagorski (1976); for the U.S.—Featherman and Hauser (1978); for West Germany—Handl (1975). The matrices of observed and circulation mobility for the 22 countries are available from the authors of this paper.

Table 5. Confirmation (1) and Rejection (0) of the FJH Hypothesis for Rates Pattern (Above Diagonal) and Odds-ratios Pattern (Below Diagonal) for 16 Countries

	AU	BE	DE	FI	FR	HU	IT	JA	NO	PH	SP	SW	US	WG	WM	YU
AU	—	0	0	0	0	0	0	0	0	1	0	0	1	0	1	0
BE	0	—	0	1	0	1	1	0	0	1	1	1	1	0	1	1
DE	0	0	—	1	0	0	1	1	0	1	0	0	1	0	1	0
FI	0	0	0	—	0	1	1	0	0	1	0	1	1	0	0	1
FR	0	0	0	0	—	0	0	1	0	1	0	1	1	0	0	0
HU	1	0	1	0	0	—	0	0	1	1	0	1	1	0	0	0
IT	0	0	0	0	0	1	—	1	1	1	1	1	1	0	0	1
JA	0	0	1	0	0	1	1	—	0	0	0	0	1	0	0	1
NO	0	0	0	0	0	1	0	0	—	1	1	0	0	1	0	1
PH	0	0	0	0	0	1	0	0	0	—	0	1	1	1	0	0
SP	0	0	0	0	0	1	0	0	0	0	—	1	1	0	0	0
SW	0	0	0	0	0	0	0	0	0	0	0	—	1	0	1	1
US	0	0	0	0	0	0	0	0	0	0	0	0	—	1	0	1
WG	0	0	0	0	0	0	0	0	0	0	0	0	0	—	0	0
WM	0	0	0	0	0	1	0	0	0	0	0	0	0	0	—	0
YU	0	0	0	0	0	1	1	0	0	0	0	0	0	0	0	—

Note: AU—Australia, BE—Belgium, DE—Denmark, FI—Finland; FR—France, HU—Hungary, IT—Italy, JA—Japan, NO—Norway, PH—Philippines, SP—Spain, SW—Sweden, US—United States, WG—West Germany, WM—West Malaysia, YU—Yugoslavia.

tions analyzed.<sup>3</sup> We computed two *measures of association in the form of odds ratios*. The first measure is an odds ratio for the *nonmanual-manual* division, in which the white-collar category is compared with the collapsed blue-collar and farm categories. The measure expresses the chances of an individual of nonmanual origin inheriting his father's status rather than moving to nonmanual status compared to the chances of an individual of manual origin moving to white-collar status rather than retaining his father's manual status. The second measure, an odds ratio for the *farm-nonfarm* division, reveals the chances for relative inheritance in the farm category.

In Figure 2, the two measures provide dimensions for a metric plane in which points for the 22 countries are plotted. Contrary to the FJH hypothesis, the scatter plot does not reveal any common pattern of circulation mobility because the countries are widely dispersed in the plane along both dimensions. Moreover, the chart distinguishes clusters of countries that cross the division not only between high and low levels of industrialization, but also between market and nonmarket economies.

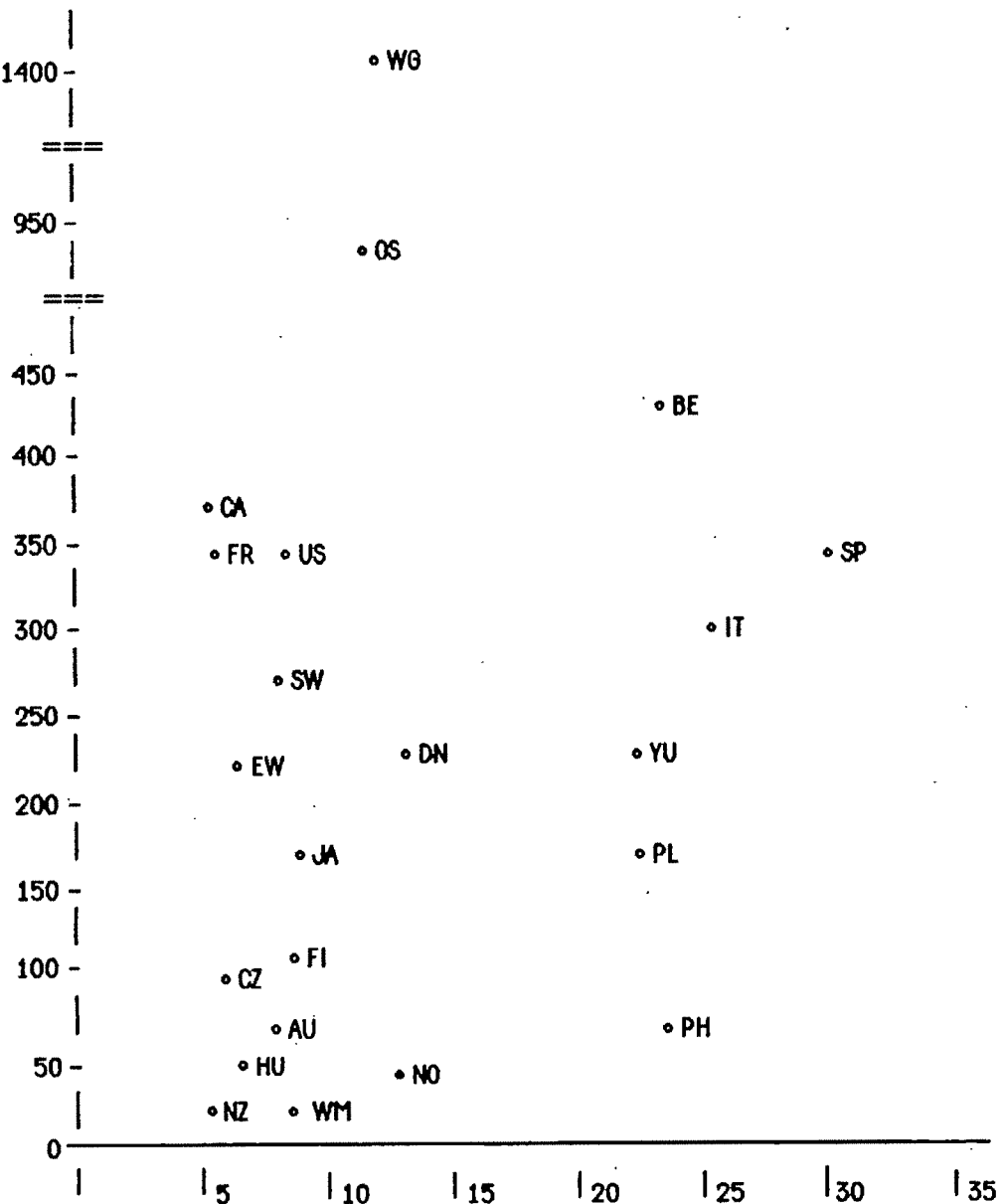
The scatter plot also suggests that associations

between origins and destinations in circulation mobility are affected by social policies, historical heritage, and cultural factors. To illustrate, if low inequality of income and wealth in countries such as New Zealand, Norway, Australia, Hungary, and Czechoslovakia is accepted as an indicator of general egalitarian social policies, then it is not surprising to find these countries in a cluster characterized by low levels of relative inheritance in both nonmanual and farm categories. Also, two countries remarkably close in historical heritage, West Germany and Austria, share the same value on one dimension but are far from the nearest cluster of five other industrial countries: the United States, England and Wales, France, Canada, and Sweden. One can easily think of a nongeographical reason why Spain and Italy are close to each other but far from, for example, Australia and New Zealand. Without overinterpreting Figure 2, it seems fair to say that its content contradicts the invariance expected under the FJH hypothesis.

For the set of 22 countries, we have examined several potential determinants of mobility ratios. The most powerful variables include indicators of economic development, the role of agriculture in the national economy, and traditional values (see Table 6).<sup>4</sup> Most of these indicators

<sup>3</sup> Using these data, we have also performed a direct test of the FJH hypothesis, as described in the previous section. This new test, based on 231 pairwise intercountry comparisons, gave the following results. For mobility patterns involving proportions and odds ratios, the FJH hypothesis was rejected in more than 78 percent of all comparisons. In the case of mobility rates, the percentage of comparisons which disconfirm the hypothesis was 29 percent for outflow rates and 25 percent for inflow rates. Generally, the results of the test for 22 countries are similar to those for 16 countries.

<sup>4</sup> Our preliminary analysis also included additional variables, such as educational enrollment (proportion of the population between ages of five and nineteen enrolled in primary and secondary education), income inequality (percent of national income going to the top five percent of households), and "social democracy" (the proportion of seats in the national legislature held by social democratic parties). We used the same sources as Grusky and Hauser (1983; 1984), replacing the unadjusted educational enrollment ratio by the adjusted one (Taylor



Note: AU—Australia 1973, OS—Austria 1978, BE—Belgium 1968, CA—Canada 1973, CZ—Czechoslovakia 1967, DN—Denmark 1972, EW—England and Wales 1972, FI—Finland 1972, FR—France 1970, HU—Hungary 1973, IT—Italy 1974, JA—Japan 1975, NZ—New Zealand 1976, NO—Norway 1972, PH—Philippines 1968, PL—Poland 1972, SP—Spain 1968, SW—Sweden 1974, US—United States 1973, WG—West Germany 1971, WM—West Malaysia 1966, YU—Yugoslavia 1962.

Fig. 2. Relative Inheritance in Nonmanual Category (Horizontal Dimension) versus Relative Inheritance in the Farm Category (Vertical Dimension) for Circulation Mobility in 22 Countries

were used in previous cross-national analyses to explain circulation (exchange) mobility (Cut-

and Hudson 1972, Table 4.3). The correlation of each variable with the measure of social inheritance (for either the nonmanual category or the farm category) turned out to be low ( $r \leq .256$ ) and not statistically significant (at  $p \leq .05$ ).

right 1968; Garnier and Hazelrigg 1976; McClendon 1980; Grusky and Hauser 1983, 1984). The results of these analyses are not conclusive.

Two indicators of economic development, the gross national product per capita and energy consumption per capita, correlate negatively with the measure of nonmanual inheritance ( $r = -.533$  and  $r = -.446$ , respectively, both significant at



Table 6. Correlations of Odds Ratios for Nonmanual and Farm Categories with Indicators of Economic Development, Importance of Agriculture, and Traditionalism, for 22 Countries

Independent Variables*	Odds Ratio for	
	Nonmanual Category	Farm Category
<i>Economic development</i>		
GNP per capita	-.533*	.174
Energy consumption	-.446*	.166
<i>Importance of agriculture</i>		
Percent of GNP from agriculture	.348	-.460*
Population in large cities	-.333	.369
<i>Traditionalism</i>		
Percent of Catholics	.609**	.295
Need-for-achievement	-.412	.151

\* For definitions, see text and the following sources: Russett et al. (1964, Tables 44, 49, 56, 73) and Tylor and Hudson (1972, Tables 4.1, 5.4, 5.7).

\*  $p < .05$ .

\*\*  $p < .01$ .

$p < .05$ ). The same indicators have a positive but weak and nonsignificant relation with the measure of farm inheritance. We hypothesize that high relative inheritance in the farm category occurs in industrialized nations in which agricultural production is on the periphery of the national economic system. Accordingly, we found a negative correlation of this measure of inheritance in circulation mobility with the proportion of domestic product originating from agriculture and a positive correlation with the proportion of the population living in cities with at least 100,000 inhabitants. Both these variables explain 25 percent of the variance in the measure of farm inheritance. Their effect is substantial even if the indicators of economic development are controlled. A partial multiple correlation showing this effect is .215.

A priori, relative inheritance in the nonmanual category is likely to covary with conservative or traditional values (Smelser and Lipset 1966, pp. 23-9). In our analysis we assumed that a high proportion of Catholics and low score on the need-for-achievement scale (McClelland 1961) are indicators of "traditionalism." For 22 countries the correlation for relative inheritance in the nonmanual category and the proportion of Catholics is high ( $r = .609$ ) and statistically significant ( $p < .01$ ). The scores on the need-for-achievement scale, available for only 18 countries, substantially covary with the same measure of relative inheritance ( $r = -.412$ ). Together, these two indicators explain 56 percent of the variance in the mobility variable. Their joint impact is statistically significant even under the control of the impact of economic development. Therefore, if the indicators of social values have some validity, the null

hypothesis about the independence between traditionalism and relative inheritance in the nonmanual category must be rejected.

These results provide a strong argument *against* the thesis about the invariance of fluidity in circulation mobility—invariance with respect to important macrostructural characteristics of countries. Some variables describing economic development, the role of agriculture in the national economy, and traditional values correlate significantly with some properties of circulation mobility. This may be seen as evidence that odds ratios computed directly from circulation-mobility tables are valid. Indeed, it is not likely that poorly defined or meaningless constructs would systematically correlate with external variables at a statistically significant level.

## CONCLUSIONS

We formulated the following testable operationalization of the original FJH hypothesis: "Among countries, national patterns of observed mobility are less similar than national patterns of circulation mobility." We gave precise meaning to the notions of circulation mobility, pattern of mobility, and similarity of patterns. This operationalization provided the basis for our tests.

Since the concept of circulation mobility is crucial to the hypothesis, we have consistently defined circulation as a kind of *mobility*—that is, in terms of transitions between origins and destinations. As *circulation* mobility, we expressed it as exchanges among occupational categories. Patterns for both observed and circulation mobility were operationalized as matrices of quantities which allow one to retrieve mobility frequencies up to a scaling factor. Proportions, inflow/outflow rates, and odds ratios derived from mobility tables were elements of respective mobility patterns. Similarity of patterns was assessed in terms of the Euclidean distance.

Applying these definitions, we tested the FJH hypothesis to verify whether intercountry similarity for circulation-mobility patterns is greater than for total-mobility patterns. We performed separate tests, using the same standard set of data for 16 countries, for patterns of proportions, rates, and odds ratios. Each test overwhelmingly rejected the hypothesis on both nonstatistical and statistical grounds. We claim that the FJH hypothesis has not been supported.

Sorokin (1959) assumed that "in any society the social circulation of individuals and their distribution is not a matter of chance, but is something which has the character of necessity, which is firmly controlled by many and various institutions" (p. 207). Under this assumption one would expect that countries that differ with respect to these institutions do not have similar

characteristics of circulation mobility. To investigate the cross-national similarity in "social circulation," we applied the concept of fluidity to circulation-mobility matrices. The results show that, across countries, the index of relative inheritance in the nonmanual category correlates negatively with measures of economic development and positively with traditionalism, and that the index of relative inheritance in the farm category covaries with the peripheral role of agriculture in the national economy. Thus, fluidity in circulation mobility is related to important macrostructural variables that differentiate countries.

The FJH hypothesis is a revision of Lipset and Zetterberg's (1956; 1959) well-known proposition, because it substitutes the phrase about invariance in the observed-mobility rate for the phrase about invariance in the circulation-mobility pattern. This revision was formulated at a time when the invariance of observed-mobility patterns in Western countries was already being questioned (Jones 1969, Cutright 1968). Accumulating evidence, using the same data for more than 15 countries, is contradictory. Some researchers claim to have discovered substantial variation in circulation-mobility patterns (Hazelrigg and Garnier 1976; McClendon 1980), while others claim to have demonstrated invariance (Grusky and Hauser 1984). In recent years, the FJH hypothesis has gained increasing acceptance stemming from the application of log-linear and multiplicative modeling to smaller sets of national mobility tables. However, the need for a test which directly compares cross-national similarity in observed- (total) mobility patterns with similarity in circulation- (exchange) mobility patterns has been overlooked. Ours is the first such test. Its results show that the Featherman-Jones-Hauser hypothesis, a revision of the Lipset-Zetterberg generalization, should be rejected.

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## DOING TIME: DYNAMICS OF IMPRISONMENT IN THE REFORMIST STATE\*

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*Most explanations of official social control point either toward reform movements or the imperatives of the social system as dominantly influencing imprisonment rates, with little attempt to integrate these distinct casual processes. This study aims to disentangle these effects by arguing that the strategic behavior of official state actors plays an intervening role in the punishment process that determines the relative salience of reform and systems effects. The empirical analysis focuses on the expansion of prisons and jails in the American states between 1880 and the early 1920s. Treating reform in terms of the adoption of probation, parole, and indeterminate sentencing legislation, and treating the social system as a store of resources likely to affect institutional expansion, the analysis pursues a series of dynamic additive and interactive models. The findings support the argument that reforms introduced discretion into the social control system and allowed official actors greater freedom to adjust their behavior to shifting bureaucratic and political constraints.*

What factors determine a society's capacity for sanctioning deviant behavior? In the last 20 years or so, this question has reemerged as a central issue in the sociology of deviance, supplanting to some degree traditional research on individual causation. A major impetus for this shift has been the conviction, derived in large part from labeling theory, that deviance is produced primarily by rules that define inappropriate behavior and prescribe legitimate societal responses and only secondarily by prior differences between deviant and nondeviant actors (Becker 1963; Erikson 1966; Lemert 1967). As a result, studies have increasingly concentrated on the historical origins and consequences of the legal rules and institutions that constitute modern strategies of control, such as the criminal law, the prison, the asylum, and the welfare system.

Yet research in this area has approached a theoretical impasse from two different directions. As Humphries and Greenberg (1981) have argued, accounts of official social control fall into two broad types: "systems theories," in

which legal controls arise in response to functional need (whether of society as a whole or, in the Marxian variant, of the ruling class), and "theories of agency," in which entrepreneurial movements create new forms of deviance independent of structural context. These accounts differ sharply in their choice of dependent variables, explanatory models, and predictions about long-term trends in punishment rates. Systems models concentrate on the development of legal systems and dominant modes of sanctioning (Parsons 1951, 1964; Schwartz and Miller 1964; Rusche and Kirchheimer 1969; Chambliss 1964). Whether inspired by Durkheim or Marx, they treat sanctioning as a mechanism of societal equilibration. In this view, intermittent reforms serve only to achieve short-term adjustments to long-term socioeconomic imperatives. Thus, sanctioning rates are predicted to be stable over time, net of a variety of structural effects.

Agency models, on the other hand, are typically applied to such millennial and parochial reforms as Prohibition (Gusfield 1963), marijuana legislation (Becker 1963), and the juvenile court (Platt 1969). They tend to emphasize the arbitrary and socially constructed nature of many control reforms, often invoking a diffuse "status politics" argument that fails to address issues of power or explain why some reforms are more successful and enduring than others. They frequently predict, but seldom demonstrate, that reform causes abrupt increases in rates of sanctioning.<sup>1</sup>

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<sup>1</sup> See, e.g., critiques of Platt's (1969) analysis of juvenile court reform by Hagan and Leon (1977) and Humphries and Greenberg (1981, pp. 238-40).

Systems and agency models approach the issue of control from nearly opposite directions. From one perspective, social systems evolve solutions to problems of social disorganization as they arise; from the other, social movements invent problems to fit their preconceived solutions. To this point there has been little direct comparison of the effects of systemic constraints and entrepreneurial deform, and the clear differences in the two perspectives prevent easy synthesis. The research reported here will focus on one factor emphasized in research by Berk and his colleagues (Berk et al. 1981, 1983; Messinger et al. 1985) that strict systems and agency models tend to ignore: the strategic and organizationally self-serving behavior of official actors who administer the social control apparatus. While conventional models tend to treat the state and its agencies as passive recipients of externally generated imperatives, a growing, diverse literature suggests that they may have a decisive impact on the generation of reform initiatives as well as their implementation. At the most general level, this approach rests on a view of modern states as complex and loosely coupled systems of organizational entities that develop their own agendas and competencies and devote considerable resources to self-legitimation (Skocpol 1985). In the politically volatile area of criminal justice, official actors face a special problem of legitimation: given a chronic scarcity of resources, they are expected to appear both humanitarian and competent in response to crime. Therapeutic reforms address this problem by expanding discretionary decision making and enhancing the self-regulatory capacity of the legal system. Reforms increase the flexibility of the system as a whole by creating new agencies, policies, and sanctioning options that permit greater selectivity in the use of coercion (Abel 1981; Garland 1985; Hagan et al. 1977; Rothman 1980; Spitzer 1975; Turk 1969). Police, court, and correctional officials use their discretion to regulate the flow of offenders in response to changing bureaucratic and political exigencies (Berk et al. 1983; Bitner 1967; Black 1980; Sudnow 1965). Reforms may increase discretion further by incorporating professional entrepreneurs and their scientific diagnoses into the official control system (Becker 1963; Foucault 1977; Hagan 1979; Scull 1977). This approach suggests that sanctioning rates are determined by the ways in which official actors use strategic discretion to manage their domains of action and only indirectly by reform or socioeconomic imperatives.

This study will explore these arguments by analyzing the mutual effects of legal reform and socioeconomic resources on imprisonment in a specific historical context. I focus on three

watershed criminal justice reforms—probation, parole, and indeterminate sentence laws—and on changes in inmate populations in the American states between 1880 and 1923. These reforms are the highest achievements of the Progressive ideal of "socialized" and "individualized" justice (Boyd 1917; Mead 1918) in the area of criminal law. According to Rothman (1980), all three emerged amid growing skepticism about the efficacy of 19th century prisons and asylums. Professional organizations like the National Prison Association, the National Conference of Charities and Correction, and the National Probation Association sponsored these policies as the keys to a more scientific and therapeutic legal system. While these reforms fell far short of their therapeutic goals (U.S. National Commission 1931; U.S. Department of Justice 1939; Rothman 1980), they nevertheless increased the variability of criminal sentences and enhanced the legitimacy of the embattled criminal justice system.

While these reforms shared a common ideological foundation, they had different implications for the distribution of discretion in the criminal justice system. Probation laws had the unique potential to affect the rate of prison admissions; in effect, they increased and formalized judges' discretion to withhold sentences. Parole legislation, by contrast, created specialized bureaucratic agencies to control the rate of prison releases. If Messinger et al.'s (1985) study of California law is generalizable, parole was not initially viewed as an instrument of rehabilitation. Instead, it began as a means to relieve the governor of incessant demands for executive pardons.<sup>2</sup> Later it was used routinely to relieve prison overcrowding (Berk et al. 1983). Indeterminate sentencing laws tended to follow parole. Sentencing reform was promoted as a means to invest parole with rehabilitative potential, indeed to make imprisonment itself therapeutic by making release contingent evidence of reform (Wines 1919, ch. 10). In practice, mandatory indeterminate sentencing reduced judges' control over the length of sentences and increased the discretionary authority of prison bureaucrats. In reaction to the 19th-century heritage of harsh sentences and chronically overcrowded and underfunded prisons, these policies introduced legitimate discretion into the sentencing process, but each in a different way and with potentially varying consequences.

The literature contains few detailed hypotheses about the relative effects of agency and

<sup>2</sup> This seems to have been a common pattern in the emergence of parole laws. See the historical notes in the U.S. Justice Department's *Survey of Release Procedures* (1939).

systems factors. On logical grounds, however, it is possible to suggest three alternative causal scenarios that approximate the arguments just discussed. Following the implicit argument of agency theory, the first is that the enactment of reform legislation has a direct and independent effect on imprisonment rates. A second possibility is that imprisonment rates respond to broad structural imperatives and that reform effects operate only in service of a more general process of equilibration. This is the core of the systems theory argument; in this study I follow Stinchcombe (1965) in conceptualizing the social system as a store of resources required to mount an organized response to deviance. The third possibility, corresponding to a "strategic discretion" approach, is that reform institutionalizes new interests within the social control regime in the form of offices, roles, and responsibilities, creates new options for the deployment of resources, and thus refracts the influence of systemic factors. In this formulation, therefore, reform and resource effects are interactive. Subsequent empirical analysis will treat these scenarios as nested models. As listed here, each is a more complex specification, as well as a falsification, of the one preceding.

Two special features of the analysis deserve mention before moving on. First, I will conceptualize systemic resources from three different theoretical perspectives. Following the standard distinction made in the social control literature (Chambliss 1976; Hagan et al. 1977; Hopkins 1975; Meier 1982), I will distinguish between a Durkheimian "consensus" approach, which emphasizes general societal complexity as the driving force behind criminal sanctions, and a Marxian "conflict" approach, which emphasizes the disruptive effects of capitalist market relations and class cleavages. In addition to these models, I pursue the logic of the strategic discretion approach by exploring the organizational capacity of the state as a determinant of sanctioning rates. The argument here is that official actors adjust their behavior to local resource flows rather than to general conditions of societal development.

The second feature of the analysis is the empirical comparison of growth rates in different kinds of institutions. To gain the broadest possible picture of official sanctioning behavior, I analyze imprisonment both in state prisons and local jails (including workhouses and lock-ups). To maintain a standard baseline for comparisons, I consider only *sentenced* inmates in both settings, ignoring, for example, inmates held in jail pending trial.

## DYNAMIC MODELS OF IMPRISONMENT

### *Hypotheses and Estimation Procedures*

Studies of imprisonment in the U.S. suggest that levels of incarceration are constrained not so much by actual rates of deviance as by societal capacities for processing and confirming deviant actors (McKelvey 1936; Pontell 1984). My empirical analysis treats imprisonment as an organizational issue; that is, I assume that prisons and jails tend to grow to the limits set by environmental constraints. Following previous literature on organizational expansion (Freeman and Hannan 1975; Nielsen and Hannan 1977), the first task is to define a hypothetical ceiling on the imprisonment capacity of a state,  $I^*$ , and to set out a model that describes its dependence on exogenous conditions. Because we lack any *a priori* evidence to the contrary, we can assume that the relationship is linear:

$$I^* = a' + c'X + d'POP \quad (1)$$

Equation (1) proposes that the maximum "capacity to punish" (Pontell 1984) is set by the size of the population ( $POP$ ) and some exogenous variables ( $X$ ), representing both reform measures and resource constraints.

One advantage of this model is its flexibility. Following Durkheim ([1938] 1964) and Blumstein et al. (1977), one may interpret equation (1) as an equilibrium model in which imprisonment adjusts in stable ways to population and, perhaps, to other exogenous factors. Recent research by Berk and his colleagues (Berk et al. 1981, 1983; Rauma 1981; see also Greenberg 1977) has shown both the logical and empirical weaknesses of this "stability of punishment" hypothesis. In this research, I will not address it directly. But it is not necessary to treat equation (1) as a stable equilibrium model. As Tuma and Hannan (1984, pp. 338-39) have argued,  $I^*$  can be interpreted more simply as the maximum level of punishment that can be sustained by exogenous factors. Because I am concerned mainly with exogenous effects, my primary attention will be to the parameters associated with the variables in  $X$ .

In historical terms it is impractical to assume that  $I^*$  is ever achieved, since exogenous conditions vary over time, and official actors cannot respond instantaneously to reform legislation or shifts in the availability of resources. It is more realistic to suggest that at any given time systems of control are expanding or contracting to close the gap between the current level of imprisonment,  $I_t$ , and the target,  $I^*$ . For convenience one may assume that the task of adjustment is a continuous-time process, even though in practice it may be driven by annual funding cycles or decisions to admit or release

inmates made weekly or daily. This process can be portrayed in a linear partial-adjustment model:

$$dI/dt = b'(I_t^* - I_t) \quad (2)$$

The parameter  $b'$  gives the rate at which the system expands or contracts to meet its carrying capacity. The higher the absolute value of  $b'$ , the more responsive the system is to external contingencies. Implicit in this formulation is the notion that different systems of incarceration vary in their ability to respond to changes in the environment. In subsequent analysis it will be important to compare the relative responsiveness of prisons and jails.

Note, however, that equation (2) suggests no explicit role for environmental factors. They can be incorporated in the form of exogenous variables by substituting (1) into (2), yielding the full dynamic model:

$$dI_t/dt = b'a' - b'I + b'c'X + b'd'POP \quad (3)$$

In substantive terms, equation (3) proposes that the instantaneous rate of growth (or decline) in imprisonment is a function of the current number of inmates, some exogenous factors, and population. Since instantaneous rates of change are unobservable, equation (3) cannot be estimated directly. Coleman (1968) has shown that one solution to equation (3) is a linear regression equation with a lagged dependent variable on the right-hand side:

$$I_t = a + bI_{t-k} + cX + dPOP \quad (4)$$

Model (4) can be estimated by pooling a minimum of three equally spaced panels of observations into a set of time-series. It yields parameters that can be used to calculate coefficients for the dynamic equation, but, as Nielsen and Rosenfeld (1981) have argued, one can also interpret equation (4) directly and substantively. While the parameter  $b'$  in equations (2) and (3) is an indicator of system responsiveness,  $b$  in equation (4) is its inverse—an indicator of inertia, or resistance to change, due to internal structural characteristics. The  $c$  and  $d$  parameters estimate the marginal impact of exogenous constraints, net of inertial effects.

Readers familiar with the research of Berk et al. (1981, 1983) will note clear differences between their strategy of estimating a partial-adjustment model of imprisonment and that given in equation (4). Part of this difference arises from the fact that I have conceptualized imprisonment as an organizational phenomenon and seek to take advantage of a modeling approach that is already widely used in the

macro-organizational literature. Another difference is that, as explained above, I do not address a strict "stability of punishment" hypothesis, an argument to which the Berk et al. models are explicitly directed. But most of the difference concerns our different sources of data. This study uses four waves of panel data, and theirs a much longer time-series from a single state. While Berk et al. use a series of lagged regressors to tame the residuals in their models, this strategy is impractical in a panel study where only a few observations are available. I describe below an alternative strategy for dealing with autocorrelation.

Earlier discussion identified three models of imprisonment that are of interest: one containing reform effects only; a second containing reform and resource effects; and a third in which reform and resources interact. These models can be specified as direct extensions of equation (4). The reform effects model has the form

$$I_t = a + bI_{t-k} + cREF_{t-k} + dPOP_t \quad (5)$$

in which  $I$  is the number of sentenced inmates in prisons or jails,  $REF$  signifies the existence of a probation, parole, or indeterminate sentence law, and  $POP$  is a control for total state population.  $REF$  represents a set of dummy variables that take the value 1 if a given law is in effect at  $t-k$ , and 0 otherwise. The  $REF$  dummies are lagged by one observation (10 years) on the grounds that any major reform requires some lead time before its effects are felt. Supplementary tests using five-year lags and no lags (instantaneous effects) showed that this specification achieves the strongest results.

It is not clear a priori what effects to expect from reform variables. Reforms could have slowed the growth of imprisonment by providing alternatives to incarceration, routinizing early releases, and shortening minimum sentences. On the other hand, each of these reforms expanded the net surveillance capacity of the state; thus they could have encouraged imprisonment if offenders who otherwise would have remained free were imprisoned for infractions of probation and parole rules. The best empirical evidence of this is in Berk and his colleagues' studies of imprisonment in California. They found no effect of probation (Berk et al. 1981) and a negative effect of parole on rates of prison expansion (Berk et al. 1983). Since, as the authors note, California may be unusual in its administration of sentencing reforms, and since they did not study jails at all, their findings are only suggestive. In general, I will treat equation (5) as an exploratory model. But, based on what is known about the content of these reforms, it is possible to hypothesize that the magnitude of their effects—whether positive or negative—

will vary systematically between prisons and jails. Since probation is a sentencing option at the discretion of the judge, it is likely that it had its greatest impact (if any) on local jail populations. This effect could have worked in two ways. First, probation was probably awarded more frequently to minor offenders who would otherwise have received short sentences to local institutions. Second, since most judges were local officials, they were probably more responsive to overcrowding in jails than in prisons. Conversely, since parole legislation was administered by state parole boards and prison administrators, it is likely that its primary effect was on penitentiary capacities. Indeterminate sentence laws specified a range of time rather than a fixed sentence for a variety of major and minor crimes. Since these laws were intended as a supplement to parole laws, their major effect was probably on prison releases.<sup>3</sup>

A second set of models adds to equation (5) a term that represents systemic resources, here represented as RES:

$$I_t = a + bI_{t-k} + c_1REF_{t-k} + c_2RES_{t-k} + dPOP_t \quad (6)$$

Note that in this equation, the added resource measures are also lagged. My expectation is that decisions to build or expand institutions, like changes in sentencing policy, have a delayed effect on punishment rates. But it is well known that, beyond their projected capacity, institutions always have some elastic capacity that is realized through under- or overcrowding. The equation includes lagged values of resource measures to account for the long-term expansion of intended prison and jail capacities, as well as contemporaneous population measures to control for short-term variation in elasticity.

If, as a systems argument suggests, legal reforms are a means by which punishment rates are equilibrated to resource availability, any effects of reform variables observed in equation (5) should be washed out in this model by the overriding effects of the resource variables. In practice I will be concerned with three sets of resources that represent the alternative consensus, conflict, and strategic discretion arguments.

From the consensus argument I will focus on urbanization and the spread of literacy in the population as salient resources for the expansion of control institutions. Again following Stinchcombe (1965), it may be argued that these variables capture a generalized level of competence for organizing activity: urbanization contributes to the concentration of wealth and the

increasing density of exchange networks, and growing literacy signifies an increased capacity for learning, communication, and launching collective projects. Urbanization also suggest a more practical influence on sanctioning. In the terms of consensus theory, urban life erodes traditional normative constraints on behavior and creates new opportunities for deviance—surely an important resource.<sup>4</sup> Under either interpretation, we would expect these variables to be positively associated with imprisonment.

The conflict model is a special case of the consensus argument. Here the relevant context is not society in general, but a specifically capitalist society. The most important resources are those that signify a potential for class-based discord. The analysis to follow examines immigration and the size of the industrial wage-labor force as indicators of inequality and the growth of capitalist market relations. The substantive argument that underlies this model is a standard one in the conflict literature. Immigrant groups were perceived by elites to be in special need of containment and socialization, and, in fact, they make up a disproportionate share of the institutionalized population at the turn of the century. Wage workers are presumed by definition to be cut loose from the affective controls of kinship and community. In a developed capitalist economy, a massed working class poses a threat to economic stability that is likely to elicit a punitive response.

This model ignores the effect of unemployment on imprisonment, even though this factor is emphasized by many conflict theorists (Rusche and Kirchheimer 1969; Rusche 1978; Box and Hale 1982; Greenberg 1977; Jankovic 1977). The practical reason for this omission is that state-level data on unemployment are not available for the period covered by this study.<sup>5</sup> But I doubt that such data would be very helpful, given the structure of the analysis. The panel design of the models, with observations on independent variables spaced every 10 years, by its very nature emphasizes long-term effects on the expansion of prison capacity. Unemployment cycles are likely to have their strongest effects over the short term, on the use of *existing* capacity. It would be interesting to test this effect, but it is of a different order than the others explored here. By excluding it I am

<sup>4</sup> Neither crime nor arrest rates are analyzed directly in this study because data are not available. Their effects on imprisonment are controlled by inclusion of the lagged dependent variable (Tuma and Hannan 1984, ch. 11).

<sup>5</sup> The variable indicating the size of the industrial wage-labor force should in no way be understood as a measure of employment or unemployment. Figures reported in census documents are averages, adjusted to control for the effects of business cycles.

<sup>3</sup> Sheldon Messinger (personal communication) notes that parole and indeterminate sentencing laws rarely, if ever, applied to jail inmates.



admittedly limiting the comprehensiveness of the analysis, but not impairing the validity of the results.

Finally, I offer two substantive hypotheses about the strategic behavior of official actors. First, it seems likely that the expansion of the social control system depends not just on general resources, but on the ability of the state to extract wealth for its own purposes. Put simply, wealthy governments are more likely to build new prisons than poor governments. I will explore the effects of state government revenue on institutional expansion. I expect that state revenue will have a major positive impact on state prison growth; its effect on local jail capacity will probably be relatively weak, or perhaps negative. Second, extending the argument by Berk et al. (1983) that prisons are "self-regulating" systems, I will explore whether prisons and jails adjust their capacities to each other. The most straightforward hypothesis is that large inmate populations in one sector will contribute to expansion in the other, as judges and other administrators attempt to even out population pressures. But it is also possible that the two sectors will show inverse patterns of growth. The construction of state prison systems, for example, may represent a trend toward the centralization of control that relieved local institutions of some of their clientele. Conversely, a strong system of jails may have inhibited the growth of prisons.

Estimates of the interrelationships of reform and resource effects will be obtained by adding a multiplicative dummy-variable interaction term to the previous additive equations:

$$I_t = a + bI_{t-k} + c_1REF_{t-k} + c_2RES_{t-k} + c_3(REF \times RES)_{t-k} + dPOP_t \quad (7)$$

In substantive terms, equation (7) suggests that the effects of resource constraints vary, depending on whether states have enacted probation, parole or indeterminate sentencing laws. But again it is difficult to suggest in advance what direction those differences might take. It could be argued that reform increased the social control system's legitimacy and administrative efficiency, permitting a more effective pursuit of available resources, or that new policies created new loci of decision making that buffered the system from external contingencies. Messinger et al. (1985) argue that sentencing reform offered California officials a strategic opportunity to reduce overcrowding, and the historical literature suggests that prison overcrowding was a chronic problem nationwide. Thus we would expect the latter direction to be the case.

Here are two last notes on estimation. First, it is widely known that in pooled regression

models, OLS techniques yield inconsistent estimates because they fail to correct for autocorrelated disturbances. In models with lagged dependent variables, parameter estimates are also likely to be biased. My analysis uses a variant of a generalized least-squares approach described by Kmenta (1971, pp. 509-12), in which it is assumed that errors are first-order autoregressive, but cross-sectionally independent. Estimation proceeds in three steps. First, OLS is applied to the original data, and the resulting residuals are used to calculate estimates of  $\rho_t$ , the autocorrelation coefficient for each case. In the second step, the  $\rho_t$ 's are used to weight the data and remove its autoregressive characteristics. The last step is to reestimate the model using OLS on the transformed data.<sup>6</sup>

A second estimation problem arises from heteroskedastic residuals caused by the enormous size differences among the states. Here I follow Johnston's (1963, pp. 207-11; see also Firebaugh and Gibbs 1985) suggestion of dividing through the entire equation, with the exception of the legal reform dummy variables, by population. Thus, for example, the weighted form of equation (5), with the addition of an error term  $u_t$ , becomes

$$I_t/POP_t = a(1/POP_t) + b(I_{t-k}/POP_t) + cREF_{t-k} + d + (u_t/POP_t) \quad (8)$$

Scatterplots show that the residuals from such models are reasonably well behaved.

### Data

This study uses three kinds of state-level longitudinal data: counts of inmate populations, dates of reform legislation, and resource measures. To fulfill the requirements of pooled regression techniques, the data were collected and organized into four panels of observations spaced according to decennial census intervals from 1890 to 1920, with lagged indicators reaching back to 1880. With four panels of data

<sup>6</sup> Potential bias due to contemporaneously correlated errors are not dealt with directly here. Problems of this sort may arise whenever all the individuals in the sample are affected by a single time-specific event. For example, wars or depressions might influence the rate of imprisonment across all states. As a simple check for such effects, models reported here were re-estimated with the inclusion of dummy variables for each time period—a variant of the well-known "least-squares with constants" procedure (Kmenta 1971, p. 516; Tuma and Hannan 1984, pp. 434-38). Results were not affected in any substantial way. The estimation program used here was written using the Statistical Analysis System MATRIX procedure.

Table 1. Sentenced Adult Inmates, 1880-1923: Number and Ratio to the Population, Total and By Institutional Type

	1880	1890	1904	1910	1923
Prison inmates	30,655	44,544 <sup>a</sup>	56,437	65,829	78,210
Jail inmates	18,531	22,233 <sup>a</sup>	23,648	31,136	20,854
Total inmates	49,186	66,777	80,085	96,965	99,064
100K total population	498.39	623.91	823.01	916.38	1,094.84
Prison inmates per 100K	61.51	71.39	68.57	71.84	71.43
Jail inmates per 100K	37.18	35.63	28.73	33.98	19.05
Total inmates per 100K	98.69	107.03	97.31	105.81	90.48

Note: Figures reported here are slightly lower than census totals due to the exclusion of inmates held in federal penitentiaries, military prisons, and mental hospitals.

<sup>a</sup> Estimate (see text).

on 48 states and territories, the resulting data set contains 192 observations.

Inmate data are drawn from a series of special institutional censuses conducted by the U.S. Census Office (1888, 1895) and the Bureau of the Census (1907, 1918, 1926) in 1880, 1890, 1904, 1910, and 1923. All figures are from enumerations made on a single day, thus population figures are independent of variation in rates of admission or average sentence length. Since officials had been counting penitentiary convicts since 1850, there are some grounds for trusting the completeness of these data. Jail enumerations were probably less thorough, but I see no evidence of systematically biased counts. While criteria for enumerating and reporting different categories of inmates varied from one census to another, it was possible to recover counts of one set of inmates—sentenced prisoners, exclusive of those held for nonpayment of fine and those held in insane asylums or military or Federal prisons—for penitentiaries and jails at all intervals except 1890.<sup>7</sup> I estimated the 1890 figures by interpolating state counts from adjacent censuses, and scaling the result to the aggregate U.S. figure (reported in U.S. Bureau of the Census 1926, p. 7, Table 1). Finally, data for 1904 and 1923 were scaled to decennial years of 1900 and 1920 by linear interpolations.

Table 1 shows inmate data aggregated at the national level from 1880 to 1923, both in absolute numbers and as a proportion of the population. According to these figures, U.S. prison capacity more than doubled over 40 years of fairly stable growth. Per capita, prisons grew

16 percent between 1880 and 1890, and remained at about the same level after that. Jails grew only slightly during this period, with a notable surge around 1910, and their capacity declined relative to population growth. These different patterns of expansion underline the importance of distinguishing between these two imprisonment strategies.

Dates of probation, parole, and indeterminate sentence laws are taken from an exhaustive historical survey of state policies conducted by the U.S. Department of Justice (1939). As described earlier, these data are transformed into dummy variables that indicate whether each state had adopted each reform at each time ( $t-k$ ). This dummy variable approach seems inevitable given the panel structure of the data, but it is imprecise because states did not enact reforms at convenient 10-year intervals. Imagine two states, one of which passed a probation law in 1901, and the other in 1910. Both would be coded the same on the probation variable, with zeros at 1880, 1890, and 1900, and ones at 1910 and 1920. When imprisonment in 1920 is regressed on the value of the probation dummy in 1910, both states appear to have identical probation policies even though one has had 19 years to respond to the new legislation and the other only 10. Dummy coding introduces some error into the models, but it is unlikely that this error systematically biases the data, since this could occur only if states showed a patterned preference for reform earlier or later in the decade.

Systems variables are drawn from regular decennial census reports (USCO 1883, 1892; USBC 1904, 1913a, 1913b, 1923, 1933). Urbanization, literacy, immigration, and wage labor are measured respectively as the number of persons who lived in cities over 25,000 population, who were literate over age 10, who were foreign born, and who were employed for wages in manufacturing industries. State government revenue is expressed in terms of constant 1967 dollars. All resource variables as well as total population are

<sup>7</sup> The 1910 census reported sentenced inmates only in state totals, omitting separate counts for prisons and jails. Breakdowns by specific institutions did not distinguish sentenced from nonsentenced inmates. I estimated separate counts for prisons and jails on the assumption that all nonsentenced inmates and all serving time for nonpayment of fines were in local institutions (an assumption supported by other, more detailed, censuses). Subtracting total prison inmates from sentenced inmates thus leaves (estimated) sentenced jail inmates.

coded in units of 100,000.<sup>8</sup> Descriptive statistics on independent and dependent variables at each decade (excluding reform dummy variables) are reported in Appendixes A and B.

## FINDINGS

### *Reform Effects*

The first step was to explore the simple effects of reform on prison and jail expansion, controlling only for population. Results from this step are reported in equation form in Table 2 (a key to variable names appears in Appendix C).

Two reform effects emerge from this table. First, parole reform (PAR) shows a significant negative effect on the rate of prison expansion. Neither probation (PROB) nor indeterminate sentencing (IND) appear to have independent effects on prisons. This result confirms the California findings of Berk et al. (1981, 1983), and suggests that parole had a decelerative effect on incarceration nationwide. The second reform effect does not, however, fit so neatly with expectations: it appears from the jail model that the adoption of indeterminate sentencing policies significantly slowed the growth of local institutions. Probation has no apparent effect. The simplest and most plausible account of this result is that indeterminate sentence laws shortened the minimum penalty for many crimes, and speeded the flow of inmates through local institutions. Whether inmates routinely served only minimum terms, or whether jail officials actively adjusted the rate of release—to alleviate overcrowding, for example—cannot be determined from the present data.

One other pattern of effects in Table 2 deserves mention. The parameters for the lag dependent variables run counter to the prediction that prison systems are slower to respond to exogenous constraints than jails. The lag jail parameter is significantly higher than the lag prison parameter, signifying higher inertia in the more decentralized institutions. How will these estimates behave when resource measures are added into the equations?

### *Additive Resource and Reform Effects*

In the second step of the analysis, indicators of resource constraints added to equations contain-

ing the reform dummy variables. Results from the consensus models are reported in Table 3, from conflict models in Table 4, and from models focusing on state-specific resources in Table 5.

In Table 3, note the performance of the reform measures when resource effects are added to the models. In the model of prison expansion, the previously observed effect of parole drops below significance, as predicted by a systems perspective. In the jail model, however, the negative impact of indeterminate sentencing persists, and appears to be independent of resource effects. A systems interpretation is complicated further by the substantive influence of resource measures. Urbanization shows no effect on prison growth, but a strong positive effect on jails. Literacy, on the other hand, shows a powerful negative effect on prison expansion, and no influence on jails. Finally, note the parameters for the lag dependent variables. Here prisons appear slightly more inertial than jails, but this effect is insignificant because the estimates are less than two standard errors apart.

It is premature at this point to offer general interpretations of these results, but I can make two observations. First, Table 3 offers no particular support for systems theory in general, or a consensus model in particular. Second, and more substantively, it appears that prisons and jails occupied different niches than those popularly assumed. Prison systems grew fastest in the least developed states, and jails persisted longest in the most urbanized states. These findings suggest that city jails were a major source of institutional growth during this period. For a more precise definition of the niche parameters of prisons and jails, we must explore additional models.

Conflict models displayed in Table 4 show equally surprising results with resource effects. The size of the immigrant population had no effect on expansion in either sector. The wage-labor parameter in the prison equation is negative, indicating that prisons grew fastest where the industrial labor market was least developed. This effect is reversed in the jail model: the association between wage labor and jail expansion is positive and significant. This suggests a need to rethink the conflict argument. Why should prison expansion decline and jails prosper in states with large industrial working classes? One explanation may lie in the activities of the early labor movement. McKelvey (1936) observes that around the turn of the century, unions, especially the powerful Knights of Labor, opposed the use of convict labor in prison industries where it could compete with free labor, and legislators in many large states responded by severely limiting the

<sup>8</sup> Indicators were chosen with an eye toward reducing multicollinearity. In the models reported below, the highest pairwise correlation is .52 (between state revenue and lag prison inmates). Belsley, Kuh, and Welsch (1980) collinearity diagnostics were estimated for all models shown subsequently. Unless stated otherwise in the text, condition index values were below 20, indicating no degradation of the estimates due to multicollinearity.

Table 2. Pooled GLS Estimates of Reform Effects on Prison and Jail Expansion (Standard Errors in Parentheses,  $N = 192$ )

PRIS =	31.79*** (5.50)	+ .59 (.05)	LPRIS*** (5.06)	- 3.64 (5.06)	PROB (4.78)	- 12.42 (4.78)	PAR** (5.45)	+ .01 (5.45)	IND (3.57)	+ 45.04 (3.57)	POP***
JAIL =	6.17** (2.34)	+ .78 (.04)	LJAIL*** (2.55)	+ 3.11 (2.55)	PROB (2.58)	+ .53 (2.58)	PAR (3.01)	- 7.70 (3.01)	IND* (1.57)	+ 10.48 (1.57)	POP***

\*  $p < .05$ .  
 \*\*  $p < .01$ .  
 \*\*\*  $p < .001$ .

Table 3. Pooled GLS Estimates of Urbanization, Literacy, and Reform Effects on Prison and Jail Expansion (Standard Errors in Parentheses,  $N = 192$ )

PRIS =	27.90*** (5.92)	+ .73 (.05)	LPRIS*** (14.03)	- 6.65 (14.03)	URB (14.80)	- 77.97 (14.80)	LIT*** (5.32)	+ 3.26 (5.32)	PROB (4.95)	- 7.68 (4.95)	PAR (5.57)	+ 4.39 (5.57)	IND (7.09)	+ 75.31 (7.09)	POP***
JAIL =	11.64*** (2.50)	+ .69 (.05)	LJAIL*** (8.98)	+ 32.25 (8.98)	URB*** (7.10)	+ 2.22 (7.10)	LIT (2.88)	- 2.62 (2.88)	PROB (2.65)	- .94 (2.65)	PAR (3.11)	- 6.23 (3.11)	IND* (3.25)	+ 6.80 (3.25)	POP*

\*  $p < .05$ .  
 \*\*  $p < .01$ .  
 \*\*\*  $p < .001$ .

Table 4. Pooled GLS Estimates of Immigration, Wage-Labor, and Reform Effects on Prison and Jail Expansion (Standard Errors in Parentheses,  $N = 192$ )

PRIS =	33.30*** (6.47)	+ .58 (.05)	LPRIS*** (28.49)	- 10.38 (28.49)	IMM (57.22)	- 125.00 (57.22)	WAGE* (5.48)	+ 1.32 (5.48)	PROB (5.04)	- 7.05 (5.04)	PAR (5.57)	- 5.03 (5.57)	IND (4.27)	+ 52.87 (4.27)	POP***
JAIL =	11.51*** (2.97)	+ .64 (.05)	LJAIL*** (13.43)	+ 7.35 (13.43)	IMM (25.66)	+ 107.78 (25.66)	WAGE*** (2.59)	- .98 (2.59)	PROB (2.44)	- .09 (2.44)	PAR (2.87)	- 7.20 (2.87)	IND* (1.55)	+ 6.64 (1.55)	POP***

\*  $p < .05$ .  
 \*\*  $p < .01$ .  
 \*\*\*  $p < .001$ .

employment of inmates. Union pressure destroyed any possibility that penitentiary regimes would pay for themselves, and may indirectly have slowed the growth of prison systems. This interpretation is tentative, however, since it is far from clear that this effect is captured by an indicator of the sheer *number* of industrial workers.

Reform effects in Table 4 are similar to those in Table 3. When resource effects are controlled, reforms appear to have had no significant effect on prison expansion. Indeterminate sentencing still shows a significant decelerative effect on jails. Again, inertia effects are not significantly different across sectors.

Table 5 shows tests of the effects of state-specific resources on imprisonment. Again, results on the resource measures defy expectations. State revenue is negatively associated with prison expansion, and shows no effect on jails. Nor is there any sign that the two sectors influence each other: large jail populations do not appear to have encouraged prison growth, nor did large prisons encourage the expansion of jails. Once again the prison model shows no significant reform effects, while the negative effect of indeterminate sentencing on jails remains strong.

Findings from these additive models run contrary to almost all the hypotheses with which the analysis began. Jails grew fastest (or declined least) in the most urbanized and industrialized states. If there is any truth to the consensus or conflict arguments, it appears that local jails rather than prisons served as the first line of defense against social disorganization. In the three prison models, none of the resource measures is positively associated with the expansion of state prison systems. Indeed, three of them—literacy, wage labor, and state revenue—show significant negative effects. On the face of it, this means that the worst educated, least industrialized, and poorest states expanded their prisons most rapidly. This conclusion suggests that we are focusing on the wrong sets of resources, or, less plausibly, that resources of any kind are not meaningful determinants of institution-building capacity.

Another way to approach this issue is to think not just about absolute levels of resources, but also about patterns of temporal change in resource availability. One suggestive piece of evidence is that, according to Appendixes A and B, literacy rates, wage-labor markets and especially state revenues all grew rapidly during this period; across all states they grew faster than imprisonment rates or population. A second clue concerns the ranking of the states on these variables. In the early years of the study, the cases ranking lowest in literacy, wage labor, and revenue were territories and new states in

the mountain and far western regions—places where obviously there was little or no opportunity for institution building. In later observations, these cases tended to move out of the lowest ranks, to be replaced often by southern states. Third, parallel analyses of regional tendencies show that prisons expanded slowest in the north Atlantic and New England states and fastest in the western regions.<sup>9</sup> Taken together, these clues suggest a cohort effect in which newer states, starting from nothing, moved more rapidly than older states in building up their social, economic, and administrative infrastructures.

One simple way to test for this cohort effect is to create a variable, called *TIME*, which indicates the number of years a state was in the Union at each observation, and to enter that variable, along with relevant resource and reform measures, into equations predicting prison growth. Because I want to compare frontier states with all others (i.e., I am less concerned with differences among older states), *TIME* is logged. For simplicity, estimated models contained only one resource variable, *PAR*, and *TIME*, along with population and the lag dependent variable. If the negative effects of literacy, wage labor, and state revenue are, in fact, byproducts of a cohort process, these effects should fade when *TIME* is included in the models, and the parameter associated with *TIME* should be negative. Results are shown in Table 6

These estimates generally support the cohort argument. In all three, the *TIME* parameter is negative and significant. In the first model, literacy maintains a significant negative effect; apparently this variable is not tied to the institutional age of states. However, models 2 and 3 show no effects of wage-labor or state revenue once *TIME* is controlled. Together, these results suggest two conclusions. First, as hypothesized, frontier states expanded their prison systems faster than more established core states. Second, they apparently did so on the basis of anticipated rather than actual resources. In subsequent analyses, I will continue to use the *WAGE* and *REV* variables rather than the simpler, but less revealing, *TIME* measure. Keep in mind, however, that these variables represent important time-dependent effects as well as cross-sectional variation among states.

In sum, analysis of linear models has yielded no categorical support for either agency or systems theories in their pure form. The negative association between parole and prison expansion may be spurious, since it does not

<sup>9</sup> These are average tendencies. Extensive tests of regional effects using dummy variables to represent regions showed no significant associations.

Table 5. Pooled GLS Estimates of Revenue, Inmate Populations, and Reform Effects on Prison and Jail Expansion (Standard Errors in Parentheses,  $N = 192$ )

PRIS =	35.03*** (6.15)	+ .72 (.07)	LPRIS*** (.05)	-1.75 (.64)	REV** (.11)	LJAIL (.11)	+1.37 (5.75)	PROB (2.63)	-6.38 (5.28)	PAR (2.63)	+23 (5.95)	IND (3.02)	+43.18 (10.73)	POP*** (1.67)
JAIL =	4.34 (2.71)	+ .78 (.05)	LJAIL*** (.05)	- .05 (.29)	REV (.03)	LPRIS (.03)	+3.11 (2.66)	PROB (2.63)	+ .80 (2.63)	PAR (2.63)	-7.94 (3.02)	IND** (3.02)	+9.27 (1.67)	POP*** (1.67)

\*  $p < .05$ .

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .

Table 6. Pooled GLS Estimates of Time-Dependence Models of Prison Expansion (Standard Errors in Parentheses,  $N = 192$ )

1. PRIS =	23.01*** (6.89)	+ .74 (.05)	LPRIS*** (.05)	- .59.83 (14.58)	LIT*** (3.66)	PAR (2.18)	-4.67 (2.18)	TIME* (8.23)	+85.36 (8.23)	POP*** (8.23)
2. PRIS =	19.43** (7.05)	+ .67 (0.06)	LPRIS*** (0.06)	-62.91 (46.20)	WAGE (3.70)	PAR* (2.23)	-7.59 (2.23)	TIME*** (7.98)	+75.47 (7.98)	POP*** (7.98)
3. PRIS =	19.07* (7.56)	+ .76 (.06)	LPRIS*** (.06)	-74 (.53)	REV (4.10)	PAR (2.10)	-8.83 (2.10)	TIME*** (8.27)	+75.37 (8.27)	POP*** (8.27)

\*  $p < .05$ .

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .

Table 7. Pooled GLS Estimates of Significant Interaction Effects on Prison Expansion (Standard Errors in Parentheses,  $N = 192$ )

1. PRIS =	28.26*** (5.90)	+ .61 (.05)	LPRIS*** (.05)	-56.23 (19.93)	URB** (5.57)	-18.54 (5.57)	PAR*** (25.48)	+57.09 (25.48)	(URB $\times$ PAR)* (4.15)	+48.92 (4.15)	POP*** (4.15)
2. PRIS =	27.92*** (5.67)	+ .62 (.05)	LPRIS*** (.05)	-190.25 (59.71)	WAGE** (5.63)	-21.45 (5.63)	PAR*** (79.74)	+196.36 (79.74)	(WAGE $\times$ PAR)** (4.14)	+51.60 (4.14)	POP*** (4.14)
3. PRIS =	38.23*** (6.04)	+ .78 (.07)	LPRIS*** (.07)	-2.29 (.59)	REV** (15.61)	-32.75 (15.61)	*PROB* (1.36)	+2.95 (1.36)	(REV $\times$ PROB)* (3.67)	+44.06 (3.67)	POP*** (3.67)
4. PRIS =	36.51*** (5.81)	+ .73 (.06)	LPRIS*** (.06)	-2.02 (.52)	REV*** (8.77)	-20.92 (8.77)	IND* (.80)	+1.75 (.80)	(REV $\times$ IND)* (3.74)	+46.91 (3.74)	POP*** (3.74)

\*  $p < .05$ .

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .

appear independent of resource effects. Under all specifications, however, indeterminate sentencing reform shows a stubborn negative association with jail expansion. Probation shows no impact on either sector. Resource measures have some profound effects on rates of incarceration, but their effects are often the reverse of what was anticipated: prison growth seems to have slowed in the older core states as it accelerated on the frontier, and jails grew most in urban-industrial states. The next step is to explore potential interactions between sentencing reforms and resource effects.

#### *Interactive Resource and Reform Effects*

To isolate the interactions of specific variables, I estimated simplified models that contained only one reform term, one resource term, and one interaction term, in addition to the lag dependent variable and population. Given three reforms, six resource variables, and two sets of penal institutions, 36 possible interactions were investigated. For simplicity, I report only interactions that are significant (at  $p \leq .05$ ) and that appear to be free of multicollinearity.<sup>10</sup> Significant interaction effects on prison expansion are shown in Table 7, and on jail growth in Table 8.

Table 7 shows four significant interaction effects on prison expansion, involving three resource variables and all three reforms. I begin the discussion with model 1, which contains urbanization and the parole dummy variable. The first point of interest in the model is the main effects: parole once again shows a significant negative main effect, and the main effect of urbanization is now significantly negative. This tells only a part of the story, however. The main effect parameter for URB is an estimate of the relationship between urbanization and prison growth when the parole variable equals zero—that is, *before* the adoption of parole legislation in each state. The interaction parameter estimates the difference between the preparole resource effect and its effect after parole was adopted. This can be clarified by deriving two separate equations, one for all observations in which PAR equals zero, and the other for all observations in which PAR

equals one. Following Kerlinger and Pedhazur (1973, pp. 251–55), the preparole equation for model 1 is found by dropping the parole and interaction terms:

$$\text{PRIS} = 28.68 + .61 \text{ LPRIS} - 56.23 \text{ URB} + 48.92 \text{ POP} \quad (9)$$

These estimates show that, in the absence of parole legislation, urbanized states expanded their prison systems at a significantly lower rate than nonurban states.

To derive the equation for observations in which PAR equals one, the PAR parameter is added to the intercept and the interaction parameter is added to URB. This gives

$$\text{PRIS} = 10.14 + .61 \text{ LPRIS} - .86 \text{ URB} + 48.92 \text{ POP} \quad (10)$$

Here the intercept drops sharply, and the parameter estimate for urbanization is not significantly different from zero (using the standard error associated with the interaction term in model 1, Table 7). Thus equation (10) shows that the enactment of parole laws slowed the rate of prison growth generally, and voided prior differences between rural and urban states. The only plausible interpretation of this latter effect is that parole had a particularly strong decelerative effect in rural states.

Model 2 shows a similar dynamic with regard to the interaction of parole and industrial wage labor. Here again, the main effect of parole is significantly negative. Wage labor shows a significant negative association with prison expansion in the absence of parole. After the adoption of parole, this association disappears (when PAR equals one, the WAGE parameter is 6.11, or effectively zero). In models 3 and 4, state revenue interacts with probation and indeterminate sentencing. The pattern of the interactions is by now familiar: both reforms had direct decelerative effects on imprisonment, and revenues were inversely related to prison expansion in the prereform period. The adoption of sentencing reforms erased differences between high- and low-revenue states.

Models 3 and 4 are difficult to interpret. For the first time, probation and indeterminate sentencing show significant effects on prisons; parole seems to drop away. The first question is whether these two interaction effects are, in fact, independent. The simplest way to test this is to insert both PROB and IND into a single equation, along with REV and their respective interactions. Equation (11) shows the results of such a test (standard errors appear in parentheses, and all significance levels are as noted in the tables):

<sup>10</sup> Results shown here exclude four significant interactions, all involving the literacy variable, that showed troublesome collinearity diagnostics (Belsley et al. 1980). Condition indexes in these models were well over 20, and ranged as high as 35. In each case it appears that the distributions of literacy, the reform dummies, and their products are so closely intertwined that it is impossible to assess their independent effects. Thus we can draw no reliable conclusions about more complex effects of literacy on either prisons or jails.

Table 8: Pooled GLS Estimates of Significant Interaction Effects on Jail Expansion (Standard Errors in Parentheses,  $N = 192$ )

1. JAIL =	12.06*** (2.39)	+ .69 (.05)	LJAIL*** + (8.54)	37.74 (8.54)	URB*** - (3.34)	1.37 (3.34)	IND - (15.50)	37.40 (15.50)	(URB × IND)*	+ 6.95 POP*** (1.62)
2. JAIL =	2.52 (2.47)	+ .80 (.04)	LJAIL*** + (12.43)	40.37 (12.43)	IMM** + (3.79)	2.22 (3.79)	PAR - (24.31)	51.29 (24.31)	(IMM × PAR)*	+ 6.71 POP*** (1.50)
3. JAIL =	3.22 (2.22)	+ .77 (.04)	LJAIL*** + (11.07)	39.26 (11.07)	IMM*** + (5.42)	5.15 (5.42)	IND - (31.68)	82.30 (31.68)	(IMM × IND)**	+ 7.14 POP*** (1.50)
4. JAIL =	12.52*** (2.18)	+ .66 (.05)	LJAIL*** + (21.21)	137.04 (21.21)	WAGE*** + (3.82)	5.03 (3.82)	IND - (55.61)	201.38 (55.61)	(WAGE × IND)***	+ 5.42 POP*** (1.52)
5. JAIL =	6.36* (2.59)	+ .80 (.05)	LJAIL*** + (.02)	.00 (.02)	REV + 29.00 (8.96)	29.00 (8.96)	PROB** - (.73)	2.45 (.73)	(REV × PROB)***	+ 8.96 POP*** (1.60)
6. JAIL =	4.82 (2.59)	+ .78 (.05)	LJAIL*** + (.25)	.43 (.25)	REV + 9.21 (4.03)	9.21 (4.03)	PAR* - (.39)	1.37 (.39)	(REV × PAR)***	+ 7.92 POP*** (1.70)
7. JAIL =	4.92* (2.42)	+ .77 (.05)	LJAIL*** + (.24)	.43 (.24)	REV + 3.51 (4.32)	3.51 (4.32)	IND - (.40)	1.22 (.40)	(REV × IND)**	+ 8.79 POP*** (1.65)
8. JAIL =	8.21** (2.67)	+ .81 (.04)	LJAIL*** - (.02)	.00 (.02)	LPRIS - 37.29 (7.35)	37.29 (7.35)	PROB*** + (.12)	.62 (.12)	(LPRIS × PROB)***	+ 7.62 POP*** (1.54)

\*  $p < .05$ .

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .



$$\begin{aligned}
 \text{PRIS} = & 37.05^{***} + .80 \text{ LPRIS}^{***} \\
 & (5.97) \quad (.07) \\
 & -2.62 \text{ REV}^{***} \\
 & (.65) \\
 & -31.30 \text{ PROB}^* \\
 & (15.90) \\
 & + 2.85 (\text{PROB} \times \text{REV})^* \\
 & (1.38) \\
 & -19.49 \text{ IND}^* \\
 & (8.93) \\
 & + 1.68 (\text{IND} \times \text{REV})^* \\
 & (.82) \\
 & + 46.44 \text{ POP}^{***} \quad (11) \\
 & (3.75)
 \end{aligned}$$

Coefficients here are almost exactly the same as those in models 3 and 4, Table 7; probation and indeterminate sentencing appear to have had separate, but similar, influences on the effect of state revenue. At this point, it is not clear why the revenue variable operates in this fashion. The only interpretation available is the simplest one: low-revenue states had high rates of prison growth only as long as they lacked any sentencing reforms. Poorer state governments discouraged prison growth either by restricting the rate of prison admissions (through the use of probation), or by increasing the release rate (through the use of indeterminate sentencing).

To interpret the results from Table 7, it will be useful to invoke the cohort hypothesis offered earlier. Recall that the wage-labor and state revenue variables reflect developmental differences between frontier and core states. The appearance of interactions involving these factors suggests that reforms had age-dependent effects. This argument will be stronger if it can be shown that urbanization is also implicated in variation across cohorts. As a test, I only add the TIME variable to model 1 in Table 7:

$$\begin{aligned}
 \text{PRIS} = & 16.84^* + .70 \text{ LPRIS}^{***} \\
 & (7.07) \quad (.05) \\
 & -21.95 \text{ URB} \\
 & (20.35) \\
 & -15.60 \text{ PAR}^{**} \\
 & (5.49) \\
 & + 34.23 (\text{URB} \times \text{PAR}) \\
 & (26.11) \\
 & -7.86 \text{ TIME}^{***} \\
 & (2.08) \\
 & + 73.20 \text{ POP}^{***} \quad (12) \\
 & (7.78)
 \end{aligned}$$

The addition of TIME diminishes the effects of URB and the interaction term to near zero. It appears that urbanization, like wage-labor market size and state revenues, primarily reflects differences among states in their rate of incorporation into the national polity.

The estimates in Table 7 reveal a number of effects that were obscured in linear-additive models of prison expansion; yet, the results all point in roughly the same direction. By the turn of the century, prison growth had slowed in the older core states where urban populations, industrial labor markets, and official administrative capacities were best developed. The main sources of expansion were the booming new states to the west where, it appears, policymakers moved aggressively to construct penal systems that were on a par with those in more established sections of the nation. Sentencing reforms—especially parole, but also including probation and indeterminate sentencing—provided the means to decelerate prison growth across all states, but the effects of reform were particularly strong in new states. On the frontier, postreform growth rates tended to fall to a national norm.

Table 8 shows eight significant interactions related to jail expansion. These interactions involve four resource variables and, again, all three reforms. Model 1 suggests a pattern somewhat the inverse of that seen in the prison analyses: where indeterminate sentence legislation was not in effect, jails expanded faster in urban states. After the adoption of indeterminate sentencing policies, the urban-rural difference disappeared (where IND equals one, the URB parameter drops to .34). This pattern is repeated in the next three models. Models 2 and 3 reveal a previously unobserved, positive main effect of immigration on jail expansion. This effect, however, operates only in the absence of reform. It appears that among states with parole legislation (model 2) or indeterminate sentencing (model 3), immigration had no effect. In fact, however, the parole-immigration interaction is spurious. When a supplementary model (not shown) was estimated that contained both reforms and their interactions with immigration, the parole effect washed out and the indeterminate sentencing-immigration interaction remained significant. Model 4 shows that the effect of the wage-labor variable was positive only among states that lacked indeterminate sentence laws. After the adoption of such policies, states with large industrial labor markets expanded their jails at a slightly slower rate than average (or allowed their jails to decline more rapidly).<sup>11</sup> These models boil down to one fairly simple conclusion: the adoption of indeterminate sentencing laws

<sup>11</sup> This effect is probably not significant. Where IND equals one, the WAGE parameter becomes  $-63.34$ , a figure less than twice the standard error associated with the interaction term.

counteracted the accelerative influence of urbanization, immigration, and industrial labor-market expansion.

Models 5, 6, and 7 show that, as in the prison interactions, revenue effects were influenced by different reforms. Here all three reforms are implicated. Each enactment appears to have decelerated the growth of jails; the higher the revenue, the greater the deceleration. Again it is important to test whether these effects are independent. The first approach to this question was to enter all three reforms and their interactions with REV into one equation. Results showed that only the probation interaction is significant. Because indeterminate sentencing has consistently been associated with jail expansion to this point, another model was estimated that contained PROB, IND, and their interactions with REV:

$$\begin{aligned}
 \text{JAIL} = & 5.34^* + .76 \text{ LJAIL}^{***} \\
 & (2.57) \quad (.06) \\
 & + .44 \text{ REV} \\
 & \quad (.24) \\
 & + 27.70 \text{ PROB}^{**} \\
 & \quad (8.80) \\
 & - 2.25 (\text{PROB} \times \text{REV})^{**} \\
 & \quad (.72) \\
 & + 3.11 \text{ IND} \\
 & \quad (4.40) \\
 & - 1.09 (\text{IND} \times \text{REV})^{**} \\
 & \quad (.42) \\
 & + 8.37 \text{ POP}^{***} \quad (13) \\
 & \quad (1.67)
 \end{aligned}$$

Both interactions are significant, suggesting again that parole had no effect on jailing independent of indeterminate sentencing. The revenue effect again appears to be influenced by the enactment of more than one sentencing reform: in the absence of any reform, jails in high-revenue states grew slightly (but insignificantly) faster than average. State adoption of either reform turned growth rates sharply downward. One anomaly remains in this model: the main effect of probation is significant and, contrary to expectation, positive. I will return to this below.

Model 8 presents what might be the most interesting result of this analysis. This model shows a significant interaction between probation and prison populations. Probation here shows a significantly negative main effect on jail expansion, as was hypothesized early on. More important, the interaction term shows that, after the enactment of probation legislation, the number of prison inmates exerted an upward pressure on jail populations (when PROB equals one, the prison coefficient becomes .62, and the parameter is significant). A scenario to account for this finding is that by lowering jail growth across the board, probation introduced slack into the system that permitted local officials to maintain some

proportion of inmates in jails when prison populations grew too large.

The true direct effect of probation on jails remains mysterious. In equation (13) probation appears to encourage jail expansion when the interaction with revenue is controlled. Model 8 in Table 8 suggests a decelerative effect when prison growth is controlled. The test of these contradictory findings is to estimate a single model containing both interactions. Results (not shown here) are inconclusive: the probation coefficient in the combined model is negative but insignificant, and collinearity diagnostics suggest there may be some degradation of the estimates. One possible interpretation is that probation had different direct effects in different states. In some it may have provided a safety-valve to reduce the flow of inmates into jails; in others it may have been a means to widen the social control net and place more offenders at risk of incarceration. Results, in short, point to higher-order interactions that cannot be confidently explored using the present data.

## DISCUSSION

This study was motivated by a theoretical critique of dominant systems and agency models of imprisonment. This critique states that imprisonment rates are not directly determined either by social-systemic imperatives or entrepreneurial reforms. Rather, they follow from the ways in which official actors use their discretionary authority to control the flow of inmates through institutions. The analyses addressed these arguments with three causal scenarios: (1) reform independently produces sharp discontinuities in punishment rates; (2) reforms intervene only to adjust sanctioning rates to long-term systemic processes; and (3) the effects of resources and reforms are mutually contingent.

The empirical results support this critique. Linear models of reform and resource effects account poorly for changes in imprisonment rates; they yielded ambiguous results and frequently obscured important relationships. More complicated interactive models revealed that reforms worked differently on prisons and jails and across subsets of states. Moreover, these results are largely compatible with the strategic-discretion argument. Prison growth was decelerated mainly by parole reform, which routinized the discretionary release of inmates. This decelerative effect was strongest in states where prisons were expanding the fastest. Jail capacities were influenced by probation legislation, which increased discretion over rates of admission, and by indeterminate sentencing, which probably encouraged faster releases. The effect of indeterminate sentencing was also strongest where jails were most expansive. It

appears that reforms were used to contain explosive growth and relieve overcrowding.

Substantively, these findings suggest two important and largely unanticipated conclusions: prisons and jails showed nearly opposite patterns of expansion, and these patterns reveal an overriding cohort effect. Prisons grew fastest in frontier states. Despite their lack of developed resources, frontier states enjoyed significant growth advantages because of their late incorporation into the national system. They avoided the painful and expensive process of evolving an original penal strategy by simply borrowing highly rationalized ideological, administrative, and even architectural models from older states.<sup>12</sup> I believe reforms had especially powerful decelerative effects in new states because they were adopted at a relatively early stage of the institution-building process. Newer prison systems were probably more reformist from the outset—that is, frontier states enacted reforms as ideologies rather than as expedients, and inherited with them the therapeutic discourse and administrative machinery that signified an advanced approach to penalty. The pattern of jail growth was the reverse: jails endured best in the core, where prison growth was most tightly constrained, and grew least in the frontier. Where probation laws were in force, large prison populations directly increased the flow of inmates into jails. Finally, jail expansion was also encouraged by the presence of large immigrant populations, a factor that did not affect prisons at all.

The findings have two larger implications for the study of social control. First, they amplify our understanding of the history of imprisonment in the U.S. As McKelvey (1936) has

written, states that had been among the pioneers of prison reform in the 1800s lost their political will by the end of the century. Legislators responded to the antagonism of unions, which feared wage competition from prison labor, and the public, which had doubted that the penitentiary was rehabilitative. The consequences in the older northeastern states were a squeezing-off of prison appropriations, a trend toward smaller, less expensive penitentiaries, and more flexible surveillance strategies such as parole. My results are compatible with this account, but they also suggest the need to treat jails as an integral component of the penal system, not as a residual set of institutions. In particular, the findings of this study imply that local jails supplemented the limited capacity of centralized prison systems, especially in urban areas. While this interpretation is largely inferential, it underscores the need to explicate the role of jails using more detailed historical sources, focusing on the ways local officials responded to the enactment of probation and indeterminate sentencing laws.

Second, the findings contain a number of theoretical implications. The analysis attempted to extend the longitudinal research of Berk et al. (1981, 1983) by drawing comparisons across states and between prisons and jails. As I have already suggested, results support their argument that prisons (and jails) are largely "self-regulating" systems of punishment. The apparent complementarity between the two sectors suggests further that self-regulation operates not only through the formal linkages that define the structure of state prison systems, but also through informal links between local and state actors. Finally, the discovery of cohort effects on imprisonment rates expands our theoretical perspective in yet another way. The different patterns of expansion in frontier and core states show that imprisonment rates are influenced not only by management strategies enacted by social control officials, but by larger strategies of state-building as well.

<sup>12</sup> Such convergent growth of prison systems is an example of what DiMaggio and Powell (1983) refer to as "mimetic isomorphism." Huntington (1973) has written more specifically about the tendency of U.S. states to reproduce each others' institutional structures.

Appendix A. Mean Absolute Values of Dependent and Independent Variables, for States and Territories, by Decade (Percent Change in Parentheses,  $N=48$ )

	PRIS	JAIL	URB	LIT	IMM	WAGE	REV	POP
1880	638.64	386.06	1.64	6.34	1.38	.57	59.90	
1890	927.96 (+45%)	463.21 (+20%)	2.69 (+64%)	8.52 (+34%)	1.92 (+39%)	.88 (+54%)	87.64 (+46%)	13.00
1900	1104.96 (+19%)	482.29 (+4%)	4.04 (+50%)	10.74 (+26%)	2.15 (+12%)	1.10 (+25%)	132.50 (+51%)	15.69 (+21%)
1910	1371.44 (+24%)	648.67 (+34%)	5.86 (+45%)	13.71 (+28%)	2.81 (+31%)	1.38 (+25%)	224.28 (+69%)	19.09 (+22%)
1920	1569.83 (+14%)	483.90 (-25%)						21.93 (+15%)
Mean change	+25%	+8%	+53%	+29%	+27%	+35%	+56%	+19%

Note: Figures for prison and jail inmates are in units of 1; all others in units of 100,000.

Appendix B. Mean Per Capita Values of Dependent and Independent Variables, for States and Territories, by Decade (Percent Change in Parentheses,  $N=48$ )

	PRIS	JAIL	URB	LIT	IMM	WAGE	REV
1880	58.20	36.29	.102	.590	.145	.042	5.06
1890	80.48 (+38%)	34.11 (-6%)	.148 (+45%)	.650 (+10%)	.162 (+12%)	.055 (+31%)	7.22 (+48%)
1900	81.74 (+1%)	29.30 (-14%)	.177 (+20%)	.684 (+5%)	.140 (-14%)	.060 (+9%)	9.55 (+32%)
1910	80.86 (-1%)	36.03 (+23%)	.217 (+22%)	.713 (+4%)	.140 (+ -0%)	.062 (+3%)	12.94 (+35%)
1920	76.98 (-5%)	21.60 (-40%)					
Mean change	+8%	-9%	+29%	+6%	-1%	+14%	+37%

Appendix C. Variable Labels and Definitions

Variable	Definition
PRIS	Sentenced prison inmates
LPRIS	Sentenced prison inmates (lag)
JAIL	Sentenced jail inmates
LJAIL	Sentenced jail inmates (lag)
PROB	Adoption of probation (lag dummy)
PAR	Adoption of parole (lag dummy)
IND	Adoption of indeterminate sentencing (lag dummy)
URB	Persons in cities over 25,000 population (lag)
LIT	Literate persons over age 10 (lag)
IMM	Foreign-born residents (lag)
WAGE	Wage workers in manufacturing industries (lag)
REV	State government revenue in constant dollars (lag)
POP	Total population
TIME	Years since statehood at each observation

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# THE MANUSCRIPT REVIEW AND DECISION-MAKING PROCESS\*

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*Peer review and editorial decision-making processes were examined for manuscripts submitted to the American Sociological Review between 1977 and 1981. Authors' professional characteristics, manuscript characteristics, review procedures, and referees' recommendations accounted for more than 58 percent of the variance in the editors' final decisions. Authors' professional characteristics had little effect. Manuscripts reporting qualitative data analysis were less likely to receive favorable recommendations from referees. Assigning editors had considerable influence via the recommendations of the referees they selected. Averaged recommendations of peer referees and the number of revisions accounted for the most variance in the editors' final decisions.*

## INTRODUCTION

Scholarly journals have used some form of peer review for nearly three centuries (Zuckerman and Merton 1971). Peer review serves manuscript authors by certifying their claims, procedures, and evidence. It serves journal editors by distributing the responsibility for the publication decision across more than one expert peer referee. Finally, it serves scholarly disciplines by improving the quality of what appears in the journals and is read by a larger community of scholars. Beyond whatever contributions may be made to scientific knowledge, publications also affect the more immediate and practical considerations of financial reward and status recognition (Hagstrom 1974; Gaston 1978). The manuscript review and decision-making process has implications for both scientific knowledge and the scientists careers. Understandably, the process has come under critical scrutiny.

Many sociologists believe the leading journals publish only the highest quality work. Others believe those journals publish only the work of scholars from the most prestigious institutions. Still others believe the leading journals are more

likely to publish only those manuscripts that reflect the theoretical, methodological, or substantive interests of their editors. And one scholar (Becker 1986) has suggested that what gets published results from editors' responsibilities to fill their page quotas in time to meet the printer's deadline with the best manuscripts they can muster, regardless of focus.

Many of these beliefs about manuscript review and decision-making are not unique to sociological journals. Most have been expressed in one form or another as long as scholarly journals have depended on editors and expert peers to decide what to print. Some of these beliefs have been investigated within the last two decades and have been found wanting by evidence from several disciplines. We report a recent investigation of manuscripts, participants in the review and decision-making process, and the process itself at the *American Sociological Review* (hereafter ASR) between 1977 and 1981. The first section discusses some relevant issues and research in the sociology of science. The second describes the data and variables in a multistage path model analysis of some of those issues. The third section reports results of that analysis. The final section discusses some implications of our research.

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## THE MANUSCRIPT REVIEW PROCESS: SOME ISSUES AND RESEARCH

Garfield (1986) notes that while considerable opinion and anecdote abound, there has been little research on the manuscript review and decision-making process. Four issues and related research in the sociology of science are summarized below.

### *Referee Selection and Recommendations*

Editors serve as gatekeepers in a variety of ways (Crane 1967). They do so directly, although infrequently, when they decide not to review or not to accept a manuscript that expert peers have recommended for publication.<sup>1</sup> They do so indirectly, and far more frequently, when they select the particular experts who review the manuscripts. Most editors have expertise in only a few areas of their disciplines. They must rely on scholars in areas other than their own (and in their own areas too, if they are wise) to help evaluate manuscripts on which they must make the final decisions. But a manuscript can be assigned to referees who are sympathetic (or antagonistic) toward its theoretical perspective, methodological approach, or substantive findings, or it can be assigned to several referees with different points of view who are, therefore, more likely to provide different evaluations. Such assignments, in the former instance, increase the likelihood of an initial acceptance (or rejection), and, in the latter, of referee disagreement that may extend the review process.

Editors, of course, are not bound by their referees' recommendations; they may override those recommendations. When they do, particularly in areas in which they are not acknowledged experts, they risk the charges of ignorance or bias.<sup>2</sup>

Accumulating evidence suggests that agreement among peer reviewers is the exception rather than the rule (but see Zuckerman and Merton 1971). Hargens (1987) reports that disagreement among manuscript reviewers is typical. Cole, Cole, and Simon (1981, p. 885) report the same among referees of proposals to the National Science Foundation (hereafter NSF). They suggest that "the great bulk of reviewer disagreement [we] observed is probably a result of real and legitimate differences of opinion among experts about what good science is or should be," and "contrary to expectation,

there was no less consensus in the social science fields of anthropology and economics than there was in the natural sciences."<sup>3</sup> In our research, agreement between first and second reviewers of first submission manuscripts was low;  $r = .16$ .

Unlike ideal-typical judgments in scientific work, editors do not ordinarily provide referees with criteria for evaluating a manuscript and making a recommendation.<sup>4</sup> Instead, most journal editors ask referees first to evaluate a manuscript and then indicate the criteria (i.e., the justifications) for their recommendations.<sup>5</sup>

This is not to say referees' recommendations are of no consequence. Zuckerman and Merton's (1971) study of manuscripts submitted to the *Physical Review* established a high correspondence between referees' recommendations and editorial decisions. Cole, Cole, and Simon's (1981) study of peer reviews of NSF proposals reports high correlations between averaged referee recommendations and program directors' awards of funding (but see Harnad 1985). Lock's (1982) study of manuscripts submitted to the *British Medical Journal* reports high correlations between referee recommendations and editor's final disposition of the manuscripts.<sup>6</sup> Thus, any two referees may disagree about a particular manuscript, which may lead the editor to seek additional reviews. The research to date indicates editors follow the general thrust of their referees' recommendations. Recommendations, at least in the research reported to date.

### *Author's Characteristics*

Several sociologists of science have suggested that the status characteristics of authors and reviewers—where they were trained and are employed—influence the reviewers' recommendations and the editor's decision to publish to a greater extent than the merits of the manuscript under review. The argument is summarized by Zuckerman and Merton (1971, p. 82). "Judg-

<sup>1</sup> Editor's of "official" journals of scholarly associations have a special responsibility to assign for peer review virtually all manuscripts they receive. But there are limits. Referees should not be burdened with the clearly unsuitable materials most editors receive (e.g., speeches, poems, collected newspaper editorials, etc.).

<sup>2</sup> Elsewhere (Simon, Bakanic and McPhail 1986), we report an analysis of complaints to editors. Of 2,220 manuscripts sent out for peer review, 1,399 were subsequently rejected by an editor. There were 74 complaints and requests for reconsideration. The initial decision was upheld for 81 percent of these manuscripts; 13 percent of the initial decisions were overturned and the manuscripts published; the editor asked the author to revise and resubmit the remaining 6 percent of the manuscripts.

<sup>3</sup> There is some evidence that funded NSF proposals (Cole, Rubin, and Cole 1979) and published manuscripts (Peters and Ceci 1982) are subsequently evaluated quite differently by a new set of peer referees.

<sup>4</sup> We know of no research where two or more referees have been asked to use the same explicit criteria to evaluate manuscripts. We realize that some journals provide reviewers with general criteria for that task. Two studies (Chase 1970; McCartney 1969) have attempted to identify the criteria reviewers employ.

<sup>5</sup> Elsewhere (Bakanic, McPhail, and Simon 1987b), we report an investigation of referees' justifications for their recommendations.

<sup>6</sup> Lock (1982) also reports that outside referees' recommendations for the disposition of these manuscripts were far more lenient than the recommendations of *BMJ*'s in-house editorial board, sardonically dubbed "the hanging committee."



ments about the work of ranking [physical] scientists may be systematically skewed by deference, by less careful appraisals involving less exacting criteria, by self-doubts of one's own sufficient competence to criticize a great [scholar] or by fear of affronting influential persons in the field."<sup>7</sup> Some evidence also indirectly supports these claims in the social sciences. (Price 1962; Beyer 1978; Crane 1972; Pfeffer et al. 1977).

Zuckerman and Merton (1971) ranked in three strata (according to peer recognition of past scientific work), the authors and reviewers of manuscripts submitted to the *Physical Review* between 1948 and 1956. High-status authors submitted more manuscripts, got slightly quicker reviews by fewer reviewers, and were more likely to be reviewed by status peers (i.e., persons of expertise) than were intermediate and third-rank authors. The manuscripts of high status authors were also more frequently accepted for publication (90 percent) than were intermediate (86 percent) and third-rank authors (79 percent). But younger authors were more likely to have their manuscripts accepted for publication than were older authors, regardless of rank, and the rate of manuscript acceptance was virtually the same whether the referees' status was higher (58 percent acceptance), the same (60 percent), or lower than the author's (59 percent). Zuckerman and Merton conclude (1971, p. 95) that, for their data set, "the relative status of referee and author has no perceptible influence on patterns of [manuscript] evaluation."

Cole, Rubin, and Cole (1979) report from their study of peer reviews of proposals to NSF that they obtained low correlations between: applicants' previous scientific track record and the receipt of funding; applicants' length of professional careers and peer evaluation as well as receipt of funding; and the prestige of applicants' institutions and peer evaluation of their proposals. Zuckerman and Merton's conclusion seems appropriate here as well. Even so, we examined the status characteristics of manuscript authors.<sup>8</sup>

### *Manuscript Characteristics*

Other critics of the manuscript review and decision-making process have suggested more subtle forms of bias. For example, general journals claim they welcome a variety of

sociological work, but there have always been complaints that these journals are more likely to accept (or reject) manuscripts with particular theoretical perspectives, methods of data collection and analysis, or substantive focuses. There is some evidence of a relationship between the publication of a manuscript and the theory and method it employs (Snizek 1975, 1976; Snizek et al. 1981; Ritzer 1975; Law 1974). These claims have yet to be investigated for a large sample of manuscripts submitted for review by a major sociological journal. Our research provided the opportunity for such an examination.

### *The Review Process*

Review procedures and decision-making rules are another potential source of bias. In their study of patterns of evaluation in scientific and humanistic journals, Zuckerman and Merton (1971) note that journals with different decision-making rules have substantially different rejection rates. For example, humanities journals appear to prefer making Type II errors; they would rather risk rejecting a worthy manuscript than accepting a worthless one. Zuckerman and Merton established that a much smaller proportion of submitted manuscripts are published in humanities and social science than in physical science journals. Hargens (1986) reports more recent comparison of journal evaluation processes in the social and physical sciences. He concludes that the decision-making process in the social sciences employs additional reviewers, takes more time, requires more revisions from the authors, and more often results in rejection of the manuscript.<sup>9</sup>

All manuscripts may not be processed or assigned to reviewers in the same way. This varies within as well as across disciplines and journals. Manuscripts are handled differently when they are resubmitted after an initial rejection than when revised and resubmitted at the editor's request or when conditionally accepted.

Not every submitted manuscript is assigned for peer review. The discretion of editors to reject without review varies widely (Hargens 1986). In addition, full-length articles are treated differently than research notes, comments on published papers, or rejoinders. Thus, review procedures vary by type of manuscript, editorial discretion, and the manuscript's record of previous submission.

<sup>7</sup> Similarly, judgments made by great scholars of other scholars' work may be accepted more on the basis of "the great's" past accomplishments than on the cogency of the current judgment.

<sup>8</sup> An examination of referee status characteristics in this data set is reported elsewhere (Bakanic 1986).

<sup>9</sup> Thus the adequacy and impartiality of the review process may assume greater importance in the social sciences, where the odds against publication are higher from the outset.

### Summary

Sociologists of science have called attention to several potential sources of influence in the manuscript review and decision-making process. Unfortunately, no one has devised a measure of manuscript quality independent of publication. Thus we cannot assess the relative contributions of such quality and the other alleged sources of influence in the manuscript review and decision-making process. Manuscript evaluation by expert peer referees may be the best available proxy measure of quality. We report below the results of our analysis of the relative contributions of referee recommendations, and of author, manuscript, and review process characteristics, to the editor's final decision.

### DATA AND METHODS

A sample of manuscripts, accompanying peer reviews, and author-editor correspondence was drawn from 2,337 manuscripts submitted to *ASR* between 1977 and 1981. The sample includes 394 eventually published manuscripts and a random sample of 361 unpublished manuscripts.<sup>10</sup> Figure 1 diagrams the general steps in our statistical analyses.

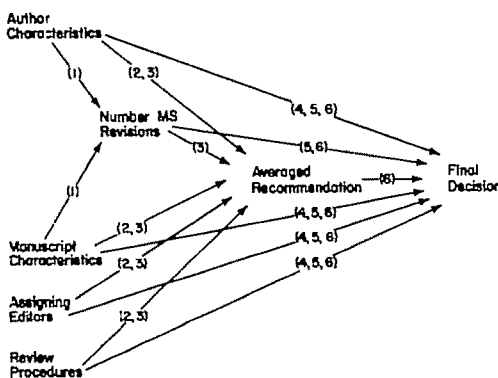


Fig. 1. Diagram of Multi-stage Path Analysis of Manuscript Review and Decision Making (numbers in parentheses refer to equations in text and in Table 1)

<sup>10</sup> Eventually published manuscripts include all versions of published manuscripts prior to final acceptance. A total of 2,337 manuscripts were received during the period in question, of which 5 percent were rejected without review or withdrawn by the author. Of the remaining 2,220 manuscripts that underwent peer review, the editors rejected 63 percent without qualification; 25 percent received an editor's request to revise and resubmit (and of these 6 percent were eventually rejected and 19 percent accepted); 3 percent were conditionally accepted; and 9 percent were unconditionally accepted.

### Dependent and Endogenous Variables

**Final disposition.** Our dependent variable is the final outcome of the review process for each submission. The values for this ordinal variable were: 1 = rejected without qualification; 2 = revise and resubmit, eventually rejected; 3 = conditionally accept, eventually rejected; 4 = revise and resubmit, not resubmitted and conditionally accept, author withdraws; 5 = revise and resubmit, eventually published; 6 = conditionally accept, eventually published; and 7 = unconditionally accept for publication. The acceptance rate for first submission at *ASR* for the five-year period under examination was approximately nine percent (226/2,337).<sup>11</sup>

**Averaged recommendation.** We constructed an average of the recommendations received by each manuscript ( $\bar{x}$  = 2.4 referees).<sup>12</sup> Reviewers' recommendations were scored: 1 = reject without qualification, or refer to another journal; 2 = request to revise and resubmit; 3 = conditionally accept; and, 4 = unconditionally accept for publication.<sup>13</sup> We summed reviewers' recommendations and divided by the number of reviewers, yielding the averaged recommendation.

**Number of revisions.** This refers to the number of times a manuscript went through the *ASR* review process. Since it was rare for a first submission manuscript to be unconditionally accepted, the number of previous submissions could affect subsequent recommendations. We noted earlier that successive submissions were subject to a slightly different review procedure.

### Exogenous Variables

**Author's professional characteristics.** We assigned scores to the first author's gender, academic rank, and professional age, the

<sup>11</sup> For all analyses reported below, the data were weighted to reflect this distribution.

<sup>12</sup> During the period in question, the editor and deputy editors met weekly to review manuscripts for which referee recommendations had been received. The editor read every manuscript without seeing the recommendations. The deputies read complete manuscripts in their areas, the introductions and summaries of all other manuscripts, and then the referees' comments and recommendations for all manuscripts. At each meeting the editor and deputies discussed each manuscript, gave their recommendations, and discussed the referees' recommendations, in that order. They then reached a consensus for the disposition of the manuscript. At no time did they quantify referee recommendations, much less calculate an average. Rather, their final decision was informed by the general thrust of those recommendations.

<sup>13</sup> For the manuscripts under consideration here, the proportions falling into these categories were, respectively, 45 percent, 21 percent, 18 percent, and 16 percent.

prestige of the institution where the author was trained, the prestige of the author's employing institution, and the author's prior participation as an ASR reviewer.<sup>14</sup> Institutional prestige was scored in accordance with a current assessment of doctoral programs in the United States (Jones et al. 1982). Prior participation as an ASR reviewer indicates the author reviewed manuscripts for ASR during the year his or her manuscript was submitted or the preceding year.

*Manuscript characteristics.* We measured the submission rate of the manuscript's substantive area<sup>15</sup> and the number of persons authoring the manuscript. We assigned ordinal scores to the method of data collection and the method of data analysis. Manuscript substantive area was classified according to 54 substantive areas recognized by the American Sociological Association, plus nine additional areas.<sup>16</sup> Coding rules insured that classification decisions followed the same criteria.<sup>17</sup> Methods of data collection were ranked: no data; archival data; primary data; and combinations of primary and archival data.<sup>18</sup> Methods of data analysis were ranked: no analysis; qualitative analysis; descriptive statistics; and advanced statistical analysis.<sup>19</sup>

*Review procedures.* We scored the number of referees evaluating the manuscript, the number of days between submission and decision, and, we constructed a control measure (NOREV) for manuscripts that were missing reviews. The number of revisions, or previous submissions, also indicates variation in review procedures but was treated as an endogenous variable because it both affected and was affected by the recommendations of referees.

*Assigning editors.* Nine editors and deputy

editors assigned manuscripts to referees during the five years (1977–81) covered by this study. Assigning editors were entered in the analyses as dummy variables. Four of these editors assigned manuscripts that were first submitted to ASR prior to 1977. Many of those manuscripts remained in the review process during 1977 and 1978. We combined the cases assigned by these four editors into a single dummy category—Retro-Editor—because each individual editor assigned so few manuscripts.

*Statistical techniques.* We estimated the coefficients for a multistage path model (see Figure 1) with a least squares solution weighted by a sampling fraction. Because the dependent variable and one of the endogenous variables are ordinal, we anticipate some questions about our use of this analytic procedure.

We also estimated a version of the model using logistic regression with a nested dichotomy. The direction and magnitude of the coefficients were similar. Since our dependent variable is ordinal and approximately normally distributed, we are convinced that weighted least squares is sufficiently robust to provide reliable estimates. We decided to report the least squares analysis because it allows more latitude in handling missing data. Logistic regression does not allow pairwise deletion of missing data. Employing list-wise deletion excluded approximately 40 percent of our cases.<sup>20</sup>

## RESULTS

*Equation 1.* The coefficients from our multistage model are presented in Table 1. The first stage of the model treated author's professional characteristics and manuscript characteristics as exogenous variables. They were used to predict variance in the number of times the revised manuscript was resubmitted to the journal.<sup>21</sup> The most striking result was the lack of significant effects. Although revised and resubmitted manuscripts had a higher probability

<sup>14</sup> Of all manuscripts in the data set, 63 percent had a single author.

<sup>15</sup> This refers to the rate of submissions by substantive area. We calculated the mean and standard deviation across all substantive area categories. Submission rates were ranked as high, medium, or low, based on each category's score relative to one standard deviation above or below the mean.

<sup>16</sup> For a complete list of substantive areas and coding instructions, see Bakanic (1986).

<sup>17</sup> We conducted reliability tests for coders' classifications of type of data collection, type of data analysis, substantive area of the manuscript, and reviewers' comments to editor and author. The lowest level of agreement between coders was 87 percent for substantive area.

<sup>18</sup> We classified as archival any data previously collected or analyzed by persons or organizations other than the authors of the manuscript.

<sup>19</sup> Slightly more than 22 percent of the manuscripts reported no data; 6 percent reported qualitative data analyses; slightly less than 10 percent reported simple descriptive statistical analyses; and the remaining 62 percent reported advanced statistical analyses.

<sup>20</sup> Two factors contributed to our missing-data problem. First, the author's professional characteristics were obtained from the directories of various scholarly associations. Some participants weren't listed in any directory we located. Second, a clerk inadvertently discarded some manuscripts, reviews, and editorial correspondence. We included a variable (NOREV = no review) to control for bias in the cases with missing reviews.

<sup>21</sup> We included the number of revisions as an endogenous variable because each submission constitutes a separate case in our analysis. Journal policy was to treat resubmissions as separate submissions rather than as a continuation of an original submission. However, since resubmissions are at a qualitatively different stage in the review process, the number of previous submissions is not an exogenous quality.

Table 1. Number Revisions Submitted, Reviewers' Averaged Recommendation, and Editor's Final Decision by Characteristics of Author, Manuscript, Assigning Editors, and Review Procedure (Unstandardized and Standardized Coefficients)

Independent Variables	Dependent Variables									
	Revisions <sup>1</sup>		Avg. Rec. <sup>2</sup>		Avg. Rec. <sup>3</sup>		Final Dcsn <sup>4</sup>		Final Dcsn <sup>5</sup>	
	$\beta$	beta	$\beta$	beta	$\beta$	beta	$\beta$	beta	$\beta$	beta
<i>Author Charact.</i>										
Rank	.028	.077	.037	.046	.013	.016	.240*	.135*	.168	.094
Gender	.066	.044	.051	.015	.005	.001	.014	.002	-.124	-.017
Age	-.005	-.089	-.005	-.028	-.000	-.000	-.060*	-.153*	-.044*	-.114*
Training	.002	.003	.002	.002	.004	.003	-.047	-.016	-.042	-.014
Employment	.019	.071	.041	.070	.029	.050	.265*	.205*	.229*	.178*
ASR reviewer	-.016	-.012	.191	.063	.196	.064	.676*	.100*	.691*	.102*
<i>Manuscript Charact.</i>										
Sub rate	.015	.019	.017	.010	.008	.004	-.010	-.002	-.038	-.010
No data	-.037	-.028	-.905	-.314	-.860	-.299	-1.620	-.255	-1.490	-.234
Primary	-.005	-.003	-.196	-.055	-.212	-.060	-.547	-.070	-.595	-.076
Combination	.079	.049	.063	.018	.008	.002	.521	.070	.357	.046
No analysis	-.092	-.069	.570	.197	.662	.215	.665	.104	.821	.128
Qualitative	-.094	-.041	-.441*	-.088*	-.371*	-.075*	-1.310*	-.120*	-1.100*	-.101*
Descriptive	-.096	-.050	-.224	-.058	-.187	-.044	-.767*	-.083*	-.596	-.064
Number authors	.038	.049	-.000	-.000	-.020	-.012	.206	.055	.145	.039
<i>Assigning Editors</i>										
Retro editors			.959*	.121*	.930*	.117*	1.790*	.102*	1.710*	.098*
Ed 1			.448*	.097*	.323*	.064*	1.060*	.095*	.561	.050
Ed 2			.287	.048	.344	.056	.253	.019	.396	.030
Ed 3			1.200*	.076*	1.130*	.072*	1.140*	.032	.937	.027
Ed 4			.208	.065	.167	.052	.321	.045	.198	.027
<i>Review Procedure</i>										
# Reviewers			-.298*	-.214*	-.199*	-.143*	-.309	-.100	-.011	-.003
# Days to decision			-.001	-.091	-.001*	-.079*	.001	.017	.001	.034
No rev. (control)	.295*	.163*	-2.600*	-.684*	-2.630*	-.679*	.368	.042	.400	.046
Number Revisions					.637*	.292*			.192*	.398*
Avg. Recommend.										
Adjusted R <sup>2</sup>	.0292		.3731		.4518		.1420		.2885	
										.5805

<sup>1-6</sup> Superscripts correspond to Equations (1) through (6) described in text.

\*  $p < .05$ .

of eventual publication, there were no significant effects for any author professional characteristics or any manuscript characteristics. The unusually low  $R$  square (.0292) describes the lack of effects.

*Equation 2.* The second stage of the model estimates the variance in the average recommendation of the referees. This stage examined the statistical effects of the editors that assigned the manuscript to referees, of the review procedure variables, and of author and manuscript characteristics. None of the author professional characteristics had statistically significant effects. This should not be surprising since referees were almost always unaware of author identity and affiliation.<sup>22</sup> One manuscript characteristic had a significant statistical effect on the referees' averaged recommendation. Manuscripts reporting qualitative data analysis were not as likely to be recommended for publication.

We have already noted that referee selection can increase the likelihood of rejection or publication. Our analysis suggests there is a considerable statistical effect of the editors who assign manuscripts on referees' averaged recommendation. Editors assigning manuscripts prior to 1977 (Retro-Editors) had a significant positive effect on referee recommendations. In addition, editors one and three also had significant positive effects. The manuscript peer review process is a double-blind procedure; that is, the authors do not know the identities of the reviewers and the reviewers are not given the identities of the authors (see footnote 13). But the editor does know the author's identity and can select referees on the basis of their expertise, the quality of their prior review work, and, conceivably, on the assumption that—given his known biases—a particular referee is likely to give a particular manuscript a harsh or lenient review. Regardless of whether editors take advantage of this opportunity to promote or torpedo a particular manuscript, the opportunity is available. We indicate below some of the reasons we believe this does not occur frequently.

Selected characteristics of the review process also yielded statistical effects on the referees' recommendations. The more days involved in reaching a decision, and the more referees, the less likely referees' recommendations were favorable. This is due, in part, to measurement procedures. Manuscripts that received split decisions were usually sent to additional refer-

ees. This increased the time involved in reaching a decision. It also raised the denominator used to calculate the average recommendation for the manuscript. The negative effect of the number of referees, although not statistically significant, supports this interpretation.

*Equation 3.* This equation in the model examined the statistical effect of the number of revisions on the averaged recommendation. With one exception, there was little change in the relationships of the independent variables to averaged recommendation. If authors had already received and complied with the editor's request to revise and resubmit a manuscript, referees were more likely to respond favorably to the revised manuscript.

*Equation 4.* The fourth equation examined the effects of the exogenous variables on the final disposition of the manuscript. There was a considerable increase in the importance of author characteristics in this equation. First, the prestige of the employing institution had a significant positive effect. If reviewers did not know the author's identity, they could not be influenced by the prestige of the employing institution. But journal editors did know the identity and the affiliation. Thus, it is conceivable (although, we think, unlikely) that assigning editors might defer to the prestige of the institution (or affiliated author) when assigning the manuscript to referees. It is also conceivable (and, we think, more likely) that prestigious institutions provide better environments and more resources for scholarly work, encourage publication, and provide more released time for its accomplishment, all of which increase the likelihood that more and higher quality manuscripts will be generated and submitted for prepublication review.

Second, an author's experience as an ASR referee significantly increased the likelihood that his or her manuscript would be published. The experience of reviewing for a journal could increase the referees' knowledge of the characteristics of manuscripts associated with publication and rejection by ASR.<sup>23</sup> But referees are ordinarily selected on the basis of their own scholarly expertise, which includes considerable experience and skill and an established record of research and publication.

Third, new Ph.D.s were more likely to publish than those who had been in the profession a long time; assistant professors were more likely to publish than full professors, associate professors and graduate students, in that order. Thus, academic rank had a positive

<sup>22</sup> On occasion, referees are so familiar with the research done in their area that they know the project from which a report is derived, if not the authors. Those referees may or may not decline to review the manuscript, and may or may not inform the editors that they know the author's identity.

<sup>23</sup> We here refer specifically to the writing style and format of presentation of manuscripts, not to the substantive area, type of data collection, or type of data analysis.

but curvilinear effect, controlling for professional age, on final disposition of the manuscript.<sup>24</sup>

Fourth, the sole manuscript characteristic yielding a statistically significant effect was the use of qualitative data analysis. The relationship was negative. A self-fulfilling prophecy may be at work here. ASR has a reputation for publishing primarily quantitative research. Therefore, savvy qualitative sociologists may choose to submit their work elsewhere, believing (incorrectly, in our view) they cannot get an impartial review at ASR. The unfortunate result is that the qualitative work submitted to ASR does not represent the full range of this important form of work that should be reviewed for publication in a general sociological journal of wide circulation.

Fifth, two assigning editor variables had significant effects. Both Retro-Editors and Editor One yielded positive relationships with final disposition. It is likely that manuscripts assigned by the Retro-Editors were continued in the review process when the journal passed to a new editor and university location. Many were split decisions or revisions, thereby increasing the probability of eventual publication. Editor One was a special case by virtue of occasionally playing the double role of both editor-in-chief and assigning editor. In the time period under examination, other editors-in-chief did not frequently assign manuscripts. And Editor One assigned fewer manuscripts than any other assigning editor (excluding the Retro-Editors).

*Equation 5.* The dependent variable for the fifth equation is the final disposition of the manuscript. But here the independent variables include the number of times the manuscript had been revised. The statistically significant relationships are consistent with the previous equations. Among the author characteristics, prestige of employing institution and experience as an ASR referee had significant positive effects. Professional age had a significant negative effect. Although academic rank had a positive effect, it was not significant in this equation.

The use of qualitative data analysis had a significant negative effect on the final disposition of the manuscript. Neither the source of

data nor the number of competing manuscripts within a substantive area had significant effects on the outcome.

Among the assigning editors only the Retro-Editors had a significant effect. As in the past three equations, the effect was positive. The significant effect of Editor One noted in Equation 4 was indirect via the number of revisions.<sup>25</sup>

The number of revisions contributed significantly to the equation. The effect was quite strong and accounted for the largest change in  $R$  square for this equation. Resubmissions had a much greater likelihood of eventual publication.

*Equation 6.* The final equation included both exogenous variables and endogenous intervening variables. Together these variables predicted over 58 percent of the variance in the final disposition of the manuscript. The independent variables predicted a substantial amount of the variance in the averaged recommendations of the referees ( $R^2 = .4518$ ) and in the final disposition of the manuscript ( $R^2 = .2885$ ). However, averaged recommendation made a strong contribution in the full equation ( $\beta = .737, p < .0001$ ). This illustrates the contribution of both referees and editor to the decision-making process. The editors solicited reviews and their decisions were consistent with the general thrust of the advice they received. Although the referees were not always in agreement, the composite provided by the averaged recommendation of the referees substantially predicts the editors' final decision.

Several author characteristics had significant statistical effects in the full equation.<sup>26</sup> The prestige of the author's employing institution continued to have a significant positive effect. This supports Crane's (1965) argument that current position is a better predictor of productivity than the institution of training (see Long, 1977).<sup>27</sup>

Professional age had a significant negative effect in this equation. The frequencies for this variable indicate that more manuscripts were submitted by authors who had been in the

<sup>25</sup> We decomposed the effects of these variables to test for their direct and indirect effects (see Alwin and Hauser 1975).

<sup>26</sup> Our analysis did not establish statistically significant effects for gender. Cole and Zuckerman (1986) suggest that, while gender fails to have statistically significant effects on single indicators of productivity, case studies of individual careers will likely yield differences in the number and kind of obstacles for men and women and their responses to those obstacles.

<sup>27</sup> There is debate over the chronological (and, therefore, the inferred causal) sequence of this relationship. Long and McGinnis (1985) suggest that author affiliation precedes and affects productivity; Cole, Cole, and Simon (1981) suggest the opposite.

<sup>24</sup> We estimated logit and probit equations for the relationships between academic rank and final disposition and between professional age and final disposition. Those procedures established a distinct S-shaped curve for the relationship between academic rank and final editorial disposition of the manuscript. We also established a less distinct S-shaped curve between professional age and final disposition. Goodness-of-fit statistics indicate acceptable models in both cases, with somewhat stronger relationships in the former than the latter.

discipline less than five years, probably a reflection of the pressure on recent Ph.D.s to publish (Cole 1979; Hargens and Hagstrom 1967; McCartney 1973; Oromaner 1977; Reskin 1977). Professional rank also yielded a significant and positive statistical effect. The effects of rank and age may be a function of self-selection by some contributors; older professionals may more frequently be invited to publish in other outlets (e.g., symposia proceedings, annual series, etc.) and can forego the lengthy and often demanding peer review required by journals.

Earlier we argued that the strength of the averaged recommendation in the full equation was due to the role referees play in the structure of the review process. If this is the case, we expect that much of the effect of the independent variables on final disposition is indirect via the referees' recommendations. There was evidence of large indirect effects for several variables that were significant in previous equations. But, in the full equation, no manuscript characteristics were among the statistically significant variables. The significant negative effect of qualitative data manuscripts, noted in Equations 4 and 5, was indirect via referees' averaged recommendation. Thus, the consistent negative effects of qualitative data analysis may reflect referees' recommendations rather than editorial bias.<sup>28</sup> There were also no significant effects in this equation for any of the assigning editors. Thus, the power of the gatekeepers is indirect via the referees. If editorial discretion affected the outcome of the review process, it is likely to have occurred at the point of assigning manuscripts to referees.

All the review process variables had strong statistical effects in Equation 6. The number of days involved in reaching a decision about the manuscript had a statistically significant positive effect. In Equation 2 this variable had a negative effect on the averaged recommendation. We noted there that the effect might have been due largely to split reviews and the resulting higher denominator when calculating the average recommendation. As evidence for our argument, we noted the negative effect of the number of referees. In Equation 6, both variables had positive effects. And, despite the greater likelihood of receiving conflicting referee recommendations, the longer a manuscript remained in the review process, the more likely it

was that the manuscript eventually was published.

One possible advantage of remaining in the review process is the opportunity to consider and incorporate referees' recommended revisions that, in turn, can affect the manuscript's final disposition. The number of revisions has a strong direct effect and an indirect effect via the averaged recommendation. Receiving a request to revise and resubmit a manuscript may improve the quality of the manuscript and, as we noted, did increase the likelihood of eventual publication. This suggests that, at least in sociology, the manuscript-review process may play an important part in the construction of what is eventually reported as social scientific knowledge.

## DISCUSSION

We have examined selected aspects of the peer review process for one major sociological journal for one period in time. We have presented evidence that the characteristics of authors, manuscripts, and the review process itself play a much smaller role in the final disposition of manuscripts than do the recommendations of expert peers.

Our research provides but slight evidence that any one area in sociology yields more publishable manuscripts than other areas. A dummy variable analysis of each of 64 areas established that manuscripts about the area of quantitative methodology were significantly associated with both the referees' averaged recommendation and the editors' final decision to publish.<sup>29</sup> We noted earlier that referees were more likely to recommend the rejection of manuscripts employing qualitative analyses. Thus, for this data set, if there was a preference for certain types of manuscripts, it was methodological and not substantive.<sup>30</sup>

<sup>29</sup> A separate dummy variable analysis of the 64 areas indicates that, when using stratification as the comparison category (because it had the largest number of manuscript submissions), few areas had significant effects on averaged recommendation or final editorial decision. Manuscripts about quantitative methodology were more likely to receive a favorable review than any other area. Suburban sociology and biosociology also had significant positive effects. Ethnomethodology and the sociology of knowledge yielded marginal negative effects. There were fewer areas with significant effects in the equation with final decision. Again, the strongest relationship was with quantitative methodology. Biosociology also had a weak positive effect. Criminal justice, which did not have a significant effect on averaged recommendation, did have a significant positive effect on final editorial decision.

<sup>30</sup> We suspect some editors welcome the opportunity to review tightly reasoned qualitative manuscripts. They

<sup>28</sup> Recall that very few qualitative analysis manuscripts were submitted for prepublication review. According to the assigning editors at ASR, the few qualitative manuscripts that were received were assigned to expert qualitative reviewers, but they did not fare as well as quantitative manuscripts assigned to quantitative reviewers.

Despite the attention author professional characteristics have received in the sociology of science literature, we found little connection between those characteristics and referees' recommendations.<sup>31</sup> The double-blind review process may diminish the effects of particularism on most referees' evaluations of most manuscripts (Beyer 1978; Pfeffer et al. 1977; Stewart 1983).

The prestige of the author's employing institution had a consistent direct effect on the final disposition of the manuscript. Authors at prestigious universities were more likely to have their manuscripts published. This may result from the expectation at such institutions that faculty will publish, the provision of released time and extraordinary resources to facilitate research leading to publication, and the recruitment and retention of older faculty with established records of scholarly productivity and younger faculty with great promise.<sup>32</sup>

There is evidence that assigning editors can indirectly influence the outcome of the decision-making process by the referees they select. We think it is unlikely that referees are selected because of the expectation that they will automatically accept or reject a particular manuscript.<sup>33</sup> The editors' problem is to get the most informed reviews possible, reviews that will yield the best manuscripts. We think it is more likely that "careful" selection of informed referees from different perspectives yields split decisions (44 percent of all first submissions in this study). That leads to additional reviews and, perhaps, increases the likelihood of eventual publication.

In Equation 6, selected characteristics of the

review procedures made strong statistical contributions to the editors' final decision. The number of days required to reach a decision and the number of times the manuscript was previously revised and resubmitted suggest that remaining in the review process increased the likelihood of eventual publication. Manuscripts receiving split decisions were usually sent to additional referees. This increased the time to reach a decision, but the longer the manuscript remained in the review process, the more likely it was eventually to be published. The most important aspect of remaining in the review process, of course, was the opportunity to respond to the referees' and editor's suggestions for revision. Revised and resubmitted manuscripts had a much greater likelihood of publication than did first submissions.

Our analysis further suggests that the low acceptance rate in sociology journals may be deceptive. Only 9 percent of the manuscripts submitted to *ASR* between 1977 and 1981 received an initial and unconditional acceptance. But nearly 29 percent were eventually published. Some of these were resubmitted as many as four times. And 75 percent of the authors who received and accepted an editor's invitation to revise and resubmit eventually published their manuscript in *ASR*.

The importance of manuscript revisions may have implications for Zuckerman's and Merton's (1971) claims about disciplinary differences in journal acceptance rates. There are still dramatic differences in the acceptance rates of social science versus physical and biological science journals (Hargens 1987). Zuckerman and Merton suggest this is evidence of corresponding differences in what constitutes scientific knowledge. But we suggest this may provide evidence of the different roles peers play in different disciplines, the timing of that participation, and the construction of research reports.

Peer review plays an important role in the revision, and reconstruction of manuscripts reviewed for publication in *ASR*. We cannot say if this is unique to the social sciences because we have no comparable data from other social, physical, and biological science journals. But there appear to be many more co-authors of research reports in physical and biological science journals than in social and behavioral science journals (Oramner 1974). Perhaps principal investigators in the physical and biological sciences include more co-authors because of a more complex division of labor during their research. Co-authors may insure that principal investigators will not report a procedure, result, or interpretation that fails to meet the expert scrutiny of a comparatively larger number of

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provide relief from the flood of good and bad quantitative manuscripts most editors receive, they are often interesting and informative, and they give editors a chance to demonstrate some balance in what they publish, particularly when they are responsible for a general journal that is supposed to report the best work across the full range of sociology.

<sup>31</sup> Elsewhere (Simon, Bakanic, and McPhail 1986), we report that, among rejected authors, tenured professors complained to the editor at a higher rate (61 percent) than their representation in the population of authors submitting manuscripts (45 percent). Untenured professors complained at about the same rate (33 percent) as their proportion (32 percent) in that population.

<sup>32</sup> Rejected authors employed at lower-prestige academic institutions complained more than those at higher-prestige institutions, and faculty trained at higher-prestige institutions complained more than those trained at lower-prestige institutions, compared to their respective proportion in the population of all authors submitting manuscripts (Simon, Bakanic, and McPhail 1986).

<sup>33</sup> Recall the low correlation between recommendations of first and second reviewers for first submission manuscripts:  $r = .16$ .



specialists.<sup>34</sup> This may be more likely to occur in the physical and biological sciences prior to the first submission of a research report for pre-publication review. In the social and behavioral sciences, where there are seldom more than two or three co-authors, expert peer participation may not come until after the journal's review and decision-making process is underway.<sup>35</sup>

The review process involves more than assigning manuscripts to expert referees. These experts frequently contribute to the revision, refocus, and refinement of research reports.<sup>36</sup> Our analysis suggests that editorial decision making is closely related to the recommendations of the referees. The editors, by and large, act consistently with the advice of the experts (but see notes 1 and 12). When editors invite authors to revise and resubmit their manuscripts, they frequently advise them to follow the recommendations of one or more expert referees. Although we have not addressed here the extent to which those recommendations are carried out (see Bakanic, McPhail, and Simon 1987a), we have provided evidence that revised and resubmitted manuscripts are much more likely to be published. In this respect, the manuscript review, revision, and resubmission processes are vital contributions to the construction of the scholarship reported in the social science literature.

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<sup>34</sup> It may also be the case that for coauthorship, as for other forms of social behavior, there are more problems coordinating the behaviors of more participants.

<sup>35</sup> Recall that 63 percent of all manuscripts were single-authored.

<sup>36</sup> This paper is no exception. Three different drafts were submitted for review. The first submission was rejected. A new version was submitted and received a revise and resubmit. The third version was conditionally accepted. Our responses to those conditions yielded the present paper. The recommendations of editors and referees for each draft were a mixture of points about which we disagreed, requests for clarification of concepts and defense of arguments, suggestions for different statistical analyses, references to literature we had overlooked, and considerable encouragement to pursue the revision. We think the paper is better now for all of that. Readers will undoubtedly pose further problems, raise new questions, and offer additional suggestions we will wish we had considered. The incorporation of these would yield yet another construction of what is reported here. There is always more than one right way!

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# INTERUNIVERSITY MOBILITY OF ACADEMIC SCIENTISTS\*

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*Previous longitudinal studies of scientists' movements in academic jobs found no evidence that research productivity affects prestige attainments. In this paper, however, we find a weak, but significant effect of productivity on the destination prestige of 274 job changes by academic physicists, chemists, mathematicians, and biologists. Major determinants of the prestige of the destination department are prestige of the prior job, prestige of the doctoral department, and the number of articles published in the six years prior to the move. Measures of citation frequency have no detectable effect, however. For promotion in rank at the occurrence of a job change, the major determinants are origin rank, professional age, and citation frequency.*

A major focus of studies of stratification has been the relative importance of achievement versus ascription in determining the allocation of rewards. The issue has had a special significance to sociologists of science, however, since Merton identified "universalism" as one of the fundamental norms in science. Universalism requires that "scientific careers be open to talent," and that "recognition and esteem accrue to those who have best fulfilled their roles, to those who have made original contributions to the body of scientific knowledge" (Merton 1973, pp. 272, 293).

There are many kinds of rewards for the performance of scientific roles; many are not controlled by a scientist's employer—for example, the esteem awarded informally by colleagues. Nevertheless, some of the most important rewards come only to those who get "good" jobs. For most scientists oriented toward basic research, a good job is a teaching position in a graduate department of a university. Academic jobs are themselves highly stratified along such dimensions as institutional prestige, salary, teaching load, academic rank, and the quality of colleagues, students, libraries, and laboratory facilities. With a few exceptions—notably climate and urban environment—such rewards tend to be highly correlated with prestige of the department or university (Hagstrom 1971; Cartter 1966). Thus, jobs in the most prestigious departments have higher mean salary, more eminent colleagues, lower teaching loads, more able students, better laboratory facilities, and

better libraries. Since good measures of the prestige of graduate departments are readily available (Cartter 1966; Roose and Andersen 1970), studies of occupational rewards in science have generally focused on this dimension.

Two findings stand out in the early literature (1965–77) on the prestige of scientists' jobs. First, the two most important determinants of prestige attainments are research productivity, as measured by numbers of publications or numbers of citations to them, and the prestige of the department in which scientists received their doctorates (Crane 1965; Hargens and Hagstrom 1967; Hagstrom and Hargens 1968; Crane 1970; Cole and Cole 1973; Allison [1976] 1980). Most studies found that these factors have approximately equal weight. Second, when both counts of articles ("quantity") and counts of citations to them ("quality") are considered, the latter has substantially more impact than the former (Cole and Cole 1973).

A positive effect of research productivity on prestige attainment is consistent with the norm of universalism. Although the effect of doctoral prestige suggests the operation of particularistic processes, alternative interpretations are possible. For example, the prestige of the doctoral department may be an indicator of scientific talent or of unpublished research, and thus may legitimately be taken into account by universalistic recruiters. This explanation should be especially relevant in the early stages of scientific careers when scientists have had little time to demonstrate their capabilities.

Virtually all the early work on scientists' prestige attainments relied on cross-sectional data. Many investigators recognized that the association between research productivity and prestige of the job could be explained in two different ways. As Crane (1965) put it

Various explanations for the success of

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scientists in certain academic environments are equally plausible, however. The best universities attract the most talented students and hire the most promising graduates. Alternatively, the institution itself, by providing opportunities and encouragement for research, may stimulate a man to greater productivity than he would exhibit in a less favorable setting.

More recent work by Long, McGinnis, and Allison suggests that it is the latter process that predominates. Using longitudinal data, they showed that prior productivity has virtually no effect on where scientists take their first positions, where they take subsequent positions, and whether and where they get postdoctoral fellowships (Long 1978; Long, Allison, and McGinnis 1979; Long and McGinnis 1981; McGinnis, Allison, and Long 1982). On the other hand, these same studies show that the prestige and sector of a scientist's job substantially affect later research productivity.

If these results are correct, then science is much less universalistic than is commonly believed. In fact, the findings are consistent with the early claim of Caplow and McGee (1958) that hiring departments pay attention only to where a candidate comes from and who recommends him, while virtually ignoring written work. Nonetheless, although superior in design to previous studies, the research of Long and his colleagues was limited in two important respects. First, they studied only biochemists, leaving open the question of whether the results hold for other fields. Second, the bulk of their data were for scientists' first jobs, when they had had only a short time to establish a publication record.<sup>1</sup> It could be argued that the first job is not an auspicious site for observing the influence of universalism. The analysis of second and later positions (Long 1978), on the other hand, was based on an extremely small sample (47 cases), raising questions of statistical reliability.

In this study, we attempt to remove both of those limitations by analyzing longitudinal data for a sample of 274 academic job changes in four disciplines: physics, chemistry, mathematics, and biology. We focus on job changes because prestige attainments can only be redistributed at such points of discontinuity. Moreover, the fact that job changes occur at isolated points in time reduces ambiguities in casual ordering. Although it would have also been desirable to compare movers with stayers,

resources were insufficient to collect data on stayers.

## DATA

### *The Sample*

The sample consisted of 274 job changes by scientists from one academic institution to another between 1961 and 1975. The distribution of these changes across disciplines was biology 27%, mathematics 27%, physics 19%, and chemistry 27%.

This sample had its origin in a survey of academic scientists conducted by Warren Hagstrom (1974). Hagstrom constructed a probability sample of 2,248 scientists in four disciplines who held faculty positions in graduate university departments in the U.S. in 1965. We searched through several editions of *American Men and Women of Science* (Cattel Press) for information about the educational background and subsequent career histories of all the scientists in Hagstrom's sample. Using the career history data, we selected all job changes meeting the following criteria:

1. Both origin and destination were four-year colleges or universities.
2. The origin department was rated for faculty quality by Roose and Andersen (1970).
3. The move occurred between 1961 and 1975, inclusive.
4. Academic rank in the origin job was assistant professor or higher.

Of the 274 changes meeting these criteria, 242 were made by unique scientists with the remaining 32 coming from 16 scientists with two moves each. We have retained these repeated changes in the analyses, but all the results reported in the tables have been replicated after restricting the sample to the first move made by each scientist.

### *Prestige of Department*

To measure prestige of both the origin and destination departments we used the ratings of faculty quality obtained by Roose and Andersen (1970). Although these were published only as rankings, we have obtained the unpublished three digit mean scores for all the departments in the study. These are properly regarded as prestige measures since they were obtained by surveying large numbers of faculty in each discipline and asking them to rate the overall quality of the faculty in each graduate department on a 6-point scale. In the analysis reported here, we have used the mean score multiplied by 100 to get a scale ranging from 0 to 500.

As noted above, all the origin jobs were necessarily in departments rated by Roose and

<sup>1</sup> Despite this fact, numbers of publications and citations prior to the first job are the best predictors of research productivity 10 years later (Long, Allison, and McGinnis 1979).

Andersen, but about 27 percent of the moves were to destination departments that were not rated in that study.<sup>2</sup> For the most part, these unrated departments were small, not well known, or only recently established. Most of these would have probably received low scores had they been rated—an inference consistent with results reported below—but some of the newer unrated departments (e.g., those at SUNY Buffalo) undoubtedly would merit more respectable ratings.

The prestige of the departments in which scientists received their doctorates was measured by the ratings in Cartter (1966). These are quite similar to the Roose and Andersen ratings, except that they were made five years earlier. Again, we have obtained the complete, unpublished three-digit scores for all the rated universities.

### *Bibliographic Measures*

For each of the 274 job changes, we collected complete bibliographic data for all journal articles published during the six years prior to the move. These were obtained from the appropriate abstracting source for each discipline. Our basic measure is the square root of the number of articles in the six-year interval.

We counted the number of times each article was cited in the *Science Citation Index* (Institute for Scientific Information), hereafter *SCI*, in the year of the job change. These counts were summed over articles to get the number of citations to each scientist's previous work. By counting citations to articles rather than to scientists, we alleviated two problems that have plagued most citation counts (Long, McGinnis, and Allison 1980). First, the citations appearing under a given scientist's name in *SCI* are only to articles for which that scientist was first author. Our procedure, by contrast, yields citation counts to all articles regardless of authorship position. Second, since *SCI* only identifies scientists by their first two initials, there is often confusion between different scientists with the same surname and initials. The problem is much less severe in locating articles in abstracting sources, since complete names are listed and because only a single discipline is involved. Once the correct articles are located, there is no longer a problem with similar names when consulting *SCI*.

To facilitate comparisons with other studies,

we also collected citation counts in the standard fashion. That is, using *SCI* for the year in which the move occurred, all citations under the scientist's name (excluding self-citations) were counted. We tried to resolve any problems with similar names by using biographical information. While these counts suffer from the difficulties noted above, they have the advantage of measuring the impact of a scientist's total bibliography, not just works published in the six years prior to the move. This could be important for eminent, older scientists who may not have published much in the years immediately preceding a job change.

Since the mean numbers of publications and citations vary greatly across fields, the counts were standardized with chemistry as a reference group. Following Allison (1980), this was done by multiplying counts in each field by a constant chosen to make the mean the same as the mean for chemists.<sup>3</sup> For publications, the constants were biology 1.06, physics 1.56, and mathematics 2.26. For citations, the constants were biology 1.31, physics 1.81, and mathematics 4.08.

### *Other Variables*

The career history data also included several other variables included in the regression models as controls: the calendar year in which the move occurred; academic rank in the origin job (coded as two dummy variables, for associate and full professor); career age (year of the move minus year that the doctorate was awarded); and field (coded as a set of three dummy variables).

## ANALYSIS

### *The Pattern of Mobility*

As expected, there is a moderate correlation between origin and destination prestige. Among the 198 moves between rated departments, the correlation was .45. This association can be seen in more detail in Table 1, which is an outflow mobility table based on four intervals of origin and destination prestige, plus a fifth category for unrated destination departments. For each level of origin prestige, there are two sets of row percentages. The first set is conditional on moving to a rated department; the

<sup>2</sup> The reason for this asymmetry in sample selection is that choosing cases by values of the independent variables will not ordinarily bias coefficient estimates. On the other hand, choosing cases by values of the dependent variable may bias the estimates (Heckman 1979; Little 1985).

<sup>3</sup> Allison (1980) suggested that disciplinary differences in productivity are essentially scale differences, i.e., the effect of being in a certain discipline is to raise or lower the expected number of articles or number of citations by a fixed percentage. He estimated these scale constants from productivity data for 2,248 scientists in chemistry, mathematics, physics, and biology.

Table 1. Prestige Rating of Destination Department by Prestige Rating of Origin Department for 274 Job Changes

Prestige of Origin Dept. *	Prestige of Destination Department					Total %	N
	400-499	300-399	200-299	0-199	Unrated		
400-499	15%	51	27	7	—	100%	47
	13%	45	23	6	13	100%	
300-399	15%	29	39	17	—	100%	84
	12%	23	31	13	21	100%	
200-299	5%	19	54	21	—	100%	82
	4%	13	38	15	30	100%	
0-199	0%	15	41	44	—	100%	61
	0%	8	23	25	44	100%	
Total	10%	28	41	21	—	100%	274
	7%	20	30	15	28	100%	

\* Examples of departments of physics at each prestige level: 400-499 (Princeton, Chicago, Columbia, Berkeley), 300-399 (Purdue, Yale, Wisconsin, Michigan), 200-299 (Penn State, Arizona, Kansas, Rice), 0-199 (Kentucky, Missouri, Texas A & M, Kansas State).

second set includes unrated departments as a destination.

The percentage of moves to unrated departments varies greatly by origin. Fully 44 percent of those leaving jobs in the lowest prestige interval (0-199) go to unrated departments, compared with only 13 percent of those leaving jobs in the highest interval. In fact, the pattern for the unrated destination departments is almost exactly what would be expected if these departments were actually in the (0-199) interval.<sup>4</sup>

It is also noteworthy that the dominant pattern is one of downward mobility. The mean for origin prestige is 304 while the mean for destination prestige is 279, a statistically significant drop of 25 points. Most of this loss was incurred by assistant professors, many of whom undoubtedly changed jobs involuntarily after being denied tenure. They suffered an average decline of 42 points on the prestige scale. On the other hand, associate and full professors, who make up 62 percent of the sample, had a statistically insignificant average decline of 5 points.

Table 2 gives correlations among the variables of major interest. After origin prestige, destination prestige is most highly correlated with doctoral prestige (.40), followed by article counts (.26) and citation counts (.23). Article counts are correlated more highly with destination than with origin, suggesting that the job change brings some improvement in fit between merit and reward. But the reverse is true for citations, with the correlation declining from .29 to .23.

<sup>4</sup> This is confirmed by fitting a log-linear model to the 4 × 5 table, constraining the 0-199 column and the unrated column to have the same interaction parameters. The likelihood ratio chi-square is only .27 with 3 degrees of freedom, indicating that these two columns can be collapsed into one with no significant detriment to the fit.

### *Determinants of Destination Prestige*

These patterns are accentuated in Table 3 (first two columns), which gives the results of an OLS regression of destination prestige on these and other variables for the 198 scientists moving to rated departments.<sup>5</sup> Although destination prestige is the dependent variable, the results are equivalent to a regression in which the dependent variable is the *difference* between destination and origin prestige. Such a regression would have coefficients that are exactly the same as those reported here for all the variables except origin prestige (whose coefficient would be smaller by 1.00). Thus, the observed effects can be interpreted as effects on the change in prestige from origin to destination (Kessler and Greenberg 1981, p. 9).

Origin prestige has the strongest effect, followed by a positive effect of doctoral prestige, a negative effect of the year of the move, and a positive effect of article counts. The decline in destination prestige with calendar year is probably the result of the rapid expansion of graduate education, primarily among less prestigious institutions, which took place during this period. To interpret the coefficient of 10.1 for article counts, recall that this independent variable is actually the square root of article counts, which implies diminishing returns from articles in their natural metric.<sup>6</sup> Thus, going from zero to one article means a gain of about

<sup>5</sup> All reported significance tests are two-sided to be conservative. OLS standard errors were checked against standard error estimates obtained with White's (1980) method, which gives consistent estimates even in the presence of heteroscedasticity. Differences were minimal.

<sup>6</sup> Tests of a quadratic specification indicated strong evidence for concave nonlinearity. The square root specification was chosen to be consistent with earlier work by Long et al., but a logarithmic transformation does equally well.

Table 2. Correlation Matrix for 198 Job Changes Between Rated Academic Employers

	Origin Prestige	Destination Prestige	Doctoral Prestige	Articles	Citations
Destination prestige	.45				
Doctoral prestige	.38	.40			
Articles	.20	.26	.11		
Citations	.29	.23	.19	.62	
Career age	.07	.09	.09	.26	.22

10 points in destination prestige, but an increase from one to four (or four to nine) articles yields only the same 10-point gain. Cumulating such increases, the expected difference in destination prestige between scientists with zero and those with 16 articles is about 40 points. To further understand the magnitude of the effect of publications on job prestige, consider the effect of changing from the least productive to the most productive sample member. The least productive scientists had zero articles, while the most productive had 73 articles during the six-year period. This is a change of 8.6 on the transformed metric. The expected change in destination prestige is 87 points, approximately the change from a position at Arizona State University to a position at University of California at Riverside in physics, or a change from UCLA to California Institute of Technology in chemistry.

There also appear to be some field differences, with biologists doing much better than those in the other three fields. Academic rank does not have a statistically significant impact, but the coefficients are in the expected direction with those at higher rank making larger gains.

The effect of citation counts is tiny, both in magnitude and statistical significance. Since previous, cross-sectional studies have reported that citation counts had a stronger effect than article counts, we explored this contrary result further. When article counts are removed from the equation, the effect of the citation measure is still far from significant. We tried various alternative transformations of citation counts, but none produced results that approached statistical significance. Since we suspected that our citation counts to publications in the previous six years might be biased against older scientists whose major works appeared earlier in their careers, we substituted counts of citations to all previous first-authored publications. This resulted in a slight increase in the coefficient and its significance level, but the *t*-statistic was still only .47. Try as we might, we could not find a plausible specification that yielded a significant effect of citation counts.

Are these results consistent across fields? When the regressions were run separately for the four fields, there were no obvious differences.

Moreover, a global Chow test (Gujarati 1978) for any differences in the regression coefficients across the four fields was not significant. We also performed more specific tests for interactions between field and article counts, origin prestige, and doctoral prestige. In no case was there any evidence of field differences in the effects of these variables.

#### *Rated vs. Unrated*

Of the 274 job changes, 75 were not included in the preceding regression because the destination departments were not rated in the Rose-Andersen (1970) study. Because these appeared to be departments of generally low visibility and prestige, we expected that similar processes would determine both destination prestige and whether or not a scientist moved to a rated department. The logistic (logit) regression reported in the right-hand side of Table 3 shows that this is generally the case.<sup>7</sup> The dependent variable was coded 1 if the move was to a rated department, otherwise 0. Two of the strongest determinants are origin prestige and number of articles, both of which have positive effects on the odds of moving to a rated department. The coefficient for citations is negative and small. The year of the move has a strong negative effect, as it did on destination prestige, and biologists are much more likely to move to rated institutions compared with those in the other three fields. Contrary to the results for destination prestige, however, we find no effect of

<sup>7</sup> It would be desirable to estimate a combined model for rated and unrated destinations, for two reasons: (a) the power of statistical tests would be increased; and (b) the exclusion of the unrated jobs may be producing a downward bias in the estimation of the coefficients for destination prestige. One way to achieve a combined model is to use the sample selection methods of Heckman (1979). We did this using LIMDEP (Greene 1984), but the results were not very satisfactory. The two-stage least squares estimates were not consistent with the maximum likelihood estimates, and both sets of estimates tended to be closer to zero than the original OLS estimates for destination prestige. Given recent criticism of Heckman's method (Little 1985), we decided not to pursue this approach further.

Table 3. Dependence of Destination Job on Origin Job and Characteristics of Scientists

Independent Variable	OLS Regression for Prestige		Logistic Regression for Rated vs. Unrated	
	Coefficient	Partial $r$	Coefficient	Partial $r^a$
Origin prestige	.328***	.37	.0064***	.20
Articles	10.095*	.17	0.487***	.22
Citations	.021	.00	-.054	-.05
Ph.D prestige	.287***	.30	.000	.00
Career age	-.566	-.04	-.078*	-.15
Year of move	-6.476***	-.27	-.284***	-.28
Field <sup>b</sup>				
Mathematics	-31.378	-.15	-1.727***	-.18
Chemistry	-31.992*	-.16	-1.726***	-.21
Physics	-29.139	-.13	-1.746***	-.20
Origin Rank <sup>c</sup>				
Assoc. prof.	8.105	.04	.112	.02
Full prof.	18.635	.06	.522	.06
Intercept	484.762		18.886	
$R^2$		.37		.28 <sup>d</sup>

<sup>a</sup> Partial correlation analog computed by  $r = \sqrt{[X^2/(n - K + X^2)]}$  where  $X^2$  is the Wald chi-square for testing  $H_0: \beta\beta = 0$ ,  $n$  is the number of observations, and  $K$  is the number of coefficients estimated. This is equivalent to a formula given by Theil (1971: p. 174) for the partial correlation in the usual linear model.

<sup>b</sup> The omitted category is biologists.

<sup>c</sup> The omitted category is assistant professors.

<sup>d</sup> Squared correlation between dependent variable and predicted value.

\*  $p < .05$ .

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .

doctoral prestige and a modest negative effect of career age.

### Rank Promotion

A change in prestige of the employing department is hardly the only consequence of moving to a new institution. Another outcome that we coded from *American Men and Women of Science* is a change in academic rank. Table 4 is a rank mobility table for the 274 job changes. Downward mobility is rare; only nine scientists moved down in rank when they changed institutions. On the other hand, of those who left as assistant professors, 60 percent moved up in rank, most to the associate level, while 52 percent of the associates moved up to full professor. This degree of upward mobility suggests that promotion (and accompanying salary increases) may be an important inducement for changing institutions.

Who gets promoted? If the norm of universalism is operative, we would expect that prior research productivity would play a major role. To test this hypothesis, we estimated a logistic regression in which the dependent variable was coded 1 if a promotion occurred, otherwise 0. Full professors were excluded from the sample since they could not be promoted. Independent variables were the same as in the previous regression analyses.

Panel A of Table 5 shows that research productivity does have an impact, but now citations rather than number of publications is

the statistically significant indicator. To give some sense of the magnitude of the citation effect, the odds of a promotion for someone with 25 citations (slightly above the mean) are about four times as great as the odds for someone with no citations. On the other hand, we find no effects for prestige of origin job or prestige of doctoral institution. Instead, the most important factors affecting promotion are rank in the origin job (associates were less likely to be promoted), and career age (older scientists were more likely to be promoted). We also observe some field differences, with mathematicians much more likely to be promoted than biologists.

As in the analysis of destination prestige, we further explored the effect of article counts to see if alternative specifications might yield a statistically significant effect. Although we tried a number of different transformations, none made a noticeable difference. However, article counts did show up as significant at the .05 level when the citation measure was deleted from the

Table 4. Destination Rank by Origin Rank

Origin Rank	Destination Rank				$N$
	Assistant	Associate	Full	Total	
Assistant	37%	59	4	100%	101
Associate	8%	40	52	100%	73
Full	0%	3	97	100%	100
Total	16%	36	51	100%	274



Table 5. Logistic Regressions for Promotion in Rank

Variable	A		B	
	Coefficient	Partial $r^a$	Coefficient	Partial $r^a$
Origin prestige	-.001	.02	.001	.04
Articles	.132	.06	.193	.09
Citations	.278**	.21	.316**	.23
Ph.D. prestige	-.001	-.03	-.001	-.05
Career age	.378***	.34	.382***	.33
Year of move	-.099	-.12	-.170*	-.18
Assoc. prof <sup>b</sup>	-2.396***	-.32	-2.537***	-.33
Field <sup>c</sup>				
Mathematics	1.817**	.22	1.613*	.19
Chemistry	.523	.08	.289	.04
Physics	.611	.09	.290	.04
Dest. prestige	—	—	-.001	-.03
Rated v. unrated	—	—	-1.308*	-.19
Intercept	3.467		9.024	
R <sup>2</sup>		.30 <sup>d</sup>		.34 <sup>d</sup>

<sup>a</sup> See Table 3, note a.

<sup>b</sup> The omitted category is assistant professors.

<sup>c</sup> The omitted category is biologists.

<sup>d</sup> Squared correlation between dependent variable and value predicted by the model.

\*  $p < .05$ .

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .

model. Given the degree of collinearity between these two productivity measures ( $r = .62$ ), there remains some doubt as to which of these indicators is more important in determining promotion.

Caplow and McGee (1958) suggested that academic job changers may trade downward prestige mobility for promotion in rank, but we find only equivocal evidence for that hypothesis. First, as already noted, there is no detectable effect of origin prestige on promotion. Second, in panel B of Table 5, we included both destination prestige and a dummy variable for rated versus unrated destination job.<sup>8</sup> With origin prestige held constant, those who move to an unrated department do have a significantly greater chance of being promoted. The odds of being promoted are about 3.5 times greater for those moving to an unrated department. On the other hand, the numerical prestige rating among those moving to rated departments has no impact on the chances of being promoted.

## DISCUSSION

Previous research by Long, Allison, and McGinnis dealt a major blow to the hypothesis that academic jobs were allocated to scientists on the

basis of their research productivity. Their longitudinal studies of biochemists found no evidence that productivity affected the prestige of first or subsequent jobs. Given previous emphasis on the role of universalism in science, their findings were highly problematic. Our results, however, offer very limited support for the productivity hypothesis. Specifically, for approximately 275 academic job changes in four disciplines in the 1960s and early 1970s, counts of previously published articles had statistically significant effects on gains or losses in job prestige. But, while the effect is significant, it is not substantively large. A change from having none to having 16 publications (from zero to four on the transformed metric) results in an expected increase in destination prestige of 40 points on a 500 point scale. Given that 80 percent of the sample have less than 16 publications, this is a major change in productivity. On the other hand, a change of 125 points in the prestige of the origin department will have the same effect on the destination prestige. A change of 140 points in the prestige of the Ph.D. department will have the same 40-point effect on the destination prestige. To obtain a prestigious job, it appears far more effective to obtain a degree from a top-rated graduate department than to be extremely productive.

Still, our results give more support for the effects of publications than Long et al. Several explanations can be considered for the disparity in results, although no definitive answer is possible. First, hiring practices in biochemistry may be less universalistic than those in the four fields studied here. This seems unlikely. Sec-

<sup>8</sup> For unrated destination jobs, the destination prestige variable was assigned the mean score for rated destination jobs. While this does not affect the coefficient of destination prestige, it has the desirable result that the coefficient for the rated-unrated dichotomy is the adjusted difference in average outcomes for rated and unrated destination jobs.

ond, the studies of biochemists dealt primarily with their first jobs. In contrast, the current study deals only with second or later jobs over a wide range of career ages. As noted earlier, it is quite plausible that hiring departments pay more attention to prior research productivity when the candidates are mature scholars with established track records. The only study that is directly comparable to that reported here is Long's (1978) analysis of 47 job changes. While he found no productivity effects, it is quite plausible that a larger sample might have yielded different conclusions. Indeed, if our sample had contained only 47 jobs changes, but had the same correlations among variables, the effect of articles on destination prestige would not have been significant.

### *Job Changes as a Market Outcome*

Although our results support the notion that hiring departments seek productive scientists, the interpretation is far from straightforward. The ambiguity is that scientists are not allocated to jobs by some master decision maker, but rather by a complex market process in which both departments and job candidates are engaged in reciprocal actions. Two features of this process cloud any observed relationship between scientists' personal characteristics and the jobs they obtain. First, most moves are voluntary, and one would expect that scientists who do not perceive a net gain will not move voluntarily. Second, scientists are not constrained to accept the most prestigious job they are offered but may instead choose on the basis of other criteria.

If the only scientists who moved were those who perceived a gain, the distribution of destination would be truncated from below. This sample selection process would tend to produce biases in coefficients relative to what would be observed if a random sample were forced to move (Heckman 1979). While there is tendency for such bias to be toward zero, it is also possible for coefficients to be inflated. Including a sample of nonmovers, together with recently developed sample selection models (Heckman 1979), might correct such biases, but we have grave doubts about the utility of such procedures (Little 1985).

More problematic is the fact that job candidates may choose among several offers, and their bases for choice might reflect quite different rewards than those studied here. Suppose, for example, that male candidates always choose the job with the highest prestige gain, while female candidates always choose the job in the largest urban environment. Then, even if hiring departments are sex-blind, we would expect to find that mobile males, on the

average, make larger prestige gains than mobile females. In general, then, the observed determinants of prestige outcomes or rank outcomes may indicate the tastes and preferences of both hiring departments and the candidates they recruit.

With these principles in minds, we turn to a detailed consideration of the estimated effects of each of the major independent variables.

### *Origin Prestige*

For those moving to rated departments, the prestige of the origin department had by far the strongest effect on destination prestige. It also had a moderate effect on whether the move was to a rated or unrated department. On the other hand, there was no effect at all on promotion in academic rank. Although it may be tempting to interpret the effect of origin prestige as an indication that hiring departments prefer candidates who are currently at high prestige departments, it is more likely that this effect is a consequence of the "frictional" preferences of the candidates themselves. No one likes to move downward on any desirable dimension; instead, scientists may simply stay put until they get an offer at a university department that is at least as prestigious as their current one. Such a process could easily produce a high correlation between origin and destination.

The fact that origin prestige does *not* affect the probability of a promotion is additional evidence for this interpretation. If hiring departments really want to recruit scientists from high-prestige departments, they should offer any available inducements to attract such people. Unlike departmental prestige, academic rank is under the control of the hiring institution and is an obvious means of attracting better candidates. Yet, those leaving prestigious departments seem to have no advantage in getting this reward.

### *Doctoral Prestige*

The effect of doctoral prestige on prestige of later jobs is surely the most persistent finding in the literature on stratification in science, and it has usually been interpreted as evidence for a particularistic strain in academic hiring. We find a strong effect in this study, too. However, for reasons we do not understand, we find no effect of doctoral prestige on whether or not the destination job had a numerical prestige rating. Since both impressionistic and empirical evidence suggest that the unrated departments are mostly at low-prestige institutions, one would expect a positive effect of doctoral prestige on the rated versus unrated dichotomy. We also

found no effect of doctoral prestige on the probability of a promotion in rank.

One obvious interpretation of the effect of doctoral prestige on destination prestige is that hiring departments actively recruit candidates with prestigious educational backgrounds. This could be either because they think this will add luster to their own institutions, or because they (rightly or wrongly) interpret such backgrounds as an indicator of scientific talent or superior training. If they really want such people, however, why don't they offer them promotions in rank in order to persuade them to come?

An alternative interpretation is one that might be described as passively particularistic. It could be that departments do not strongly prefer candidates from prestigious doctoral departments, but that these candidates are more likely to come to their attention as a result of social ties that are stratified by prestige. Such a process might produce a strong correlation between prestige ratings of doctoral and destination departments, without having any bearing on promotion in rank. Promotions, after all, are likely to occur *after* a candidate has been chosen, as an inducement to accept the offer, and thus should not necessarily be affected by social ties.

A third possible interpretation is that the effect of doctoral prestige reflects the preferences of the candidates themselves. It is well known that the movement from graduate department to first job is typically one of downward prestige mobility. If we assume that scientists experience this as a loss, it is reasonable to expect that they would be highly motivated to regain a prestige level comparable to that of their graduate experience. If they then seek out and accept offers from institutions similar to those in which they were educated, this alone would produce a correlation between doctoral and destination prestige. It would *not*, however, produce a correlation between doctoral prestige and rank promotion. In fact, if the prestige motivation is strong enough, we might expect that scientists educated at prestigious departments would tolerate downward mobility in rank in exchange for upward prestige mobility.

#### *Research Productivity*

The most noteworthy result of this study is that prestige mobility is determined, in part, by the number of articles that a scientist has recently published, but not by the number of citations to those articles or to earlier articles. This supports those who believe that science is a meritocratic institution, but it is troubling that the quantity rather than the "quality" of the work seems to affect mobility outcomes and that the effect is

smaller than that of either Ph.D. origin or current departmental prestige. The work by Cole and Cole (1973) provided convincing evidence that citation counts, for all their defects, are a strong indicator of the visibility of a scientist and the impact of his or her research on the research of others. The measure easily surpasses what has been achieved in most other efforts to measure occupational productivity. Yet, we find no evidence that a high citation count (whatever it may measure) is an advantage in getting a prestigious job.

However, we find that scientists with many citations are more likely to be promoted in rank when they change jobs, suggesting that departments (and their parent institutions) do pay some attention to this dimension of scientists' publication records. Furthermore, controlling for the citation measure, the number of articles published does not seem to play a role in the decision to promote. It is not obvious why departments should seek *quantity* of research in deciding which scientists to recruit, but reward *quality* of research in deciding what inducements to offer their leading candidates.

We must again caution that these results may also indicate the preferences of job seekers. It is possible that highly productive scientists are more motivated to seek prestigious jobs and the rewards that accompany those jobs.

#### *Suggestions for Further Research*

Long, Allison, and McGinnis claimed that research productivity does not affect a scientist's job placement, but that job placement does affect subsequent research productivity. We have looked at the first claim and found that it needs to be moderated when applied to job changes after the first job. Using the same sample of job changes, we are now re-examining the second claim. Results are still incomplete.

While most previous studies have concentrated on the prestige outcome of scientists' mobility, we believe that the study of rank changes provides an important complement. In fact, the determinants of promotion may give a less ambiguous indication of what hiring departments look for in their new recruits. The picture is incomplete, however, because we do not know much about the process by which academic promotions are awarded to those who do not change institutions. While some preliminary work has been done on this topic (Long 1977; Cole 1979), there is a real need for research that uses event-history methods (Allison 1984; Tuma and Hannan 1984) to study this reward.

Event-history methods should also be used to determine why some scientists change jobs and

some do not. While this will not directly address the process of reward allocation, it can shed considerable light on what rewards are important to scientists in their job searches (Allison [1976] 1980). Such knowledge can help greatly in interpreting the kinds of results we have reported here.

Finally, there is the question of the generalizability of our results. Our sample of job changes occurred mostly in the 1960s, with a few in the early 70s. This period was predominantly one of rapid expansion, but it ended with a downturn in academic employment opportunities. It was also a period in which equal opportunity and affirmative action requirements were beginning to be implemented and institutionalized. Would the same results be obtained today? It is possible that the more formalized recruitment practices that are now the norm would produce a more universalistic pattern of hiring. On the other hand, the generally dismal conditions that characterized the academic labor market in the late 70s and early 80s may have induced pressures to rely on social ties to an even greater extent. Only additional research can settle this question.

There is also the question of whether similar results would obtain for the social sciences. Hargens and Hagstrom (1982) reported that status attainment processes differ somewhat in political science compared with those in the natural sciences, and they interpreted those results to be a consequence of differing levels of codification and consensus. The same may be true for institutional mobility.

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# THE STABILITY OF STUDENTS' INTERRACIAL FRIENDSHIPS\*

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*In this paper we study the determinants of the stability of schoolchildren's interracial and same-race friendships. We argue that classroom organizational features and student characteristics affect the cohesiveness of social ties with consequences for friendship stability. The hypotheses are tested on longitudinal data from 375 fourth-through seventh-grade students in 16 desegregated classrooms. Descriptive and inferential analyses show that interracial friendships are almost as stable as same-race ones. Further, while the stability of interracial and same-race friendships is influenced by classroom characteristics, it is more strongly influenced by ascribed and achieved characteristics of students. We conclude that while individual characteristics of students are the strongest determinants of interracial friendship stability, schools can adopt policies and practices that promote stable friendships between black and white students.*

While social scientists have long been interested in the determinants of friendship formation, they have given surprisingly little attention to the processes that govern the longevity of friendships. This is particularly true of interracial friendships. Psychologists have identified personality and attitudinal variables that predict the formation of interracial friendships (e.g., Duck and Gilmour 1981), and sociologists have pointed to structural and organizational features of the environment that promote interracial friendships (Patchen 1982; Schofield 1982; Grant and Rothenberg 1981; Hallinan and Teixeira 1987 a, b). But while this research contributes to the theoretical understanding of race relations, it neglects the important question of how long interracial friendships persist. It is not known whether interracial friendships are transitory and basically unstable, or whether, once formed, they persist for a considerable period.

Determining the stability of interracial friendships is critical to understanding both how blacks and whites interact in different settings and what significance they attach to interracial friendships in desegregated schools. Learning how long students' interracial friendships persist should reveal the importance of school factors in influencing interracial friendships. If, for example, interracial friendships are fleeting, then

teachers' attempts to influence their formation may be less important than their efforts to sustain friendships that already exist. Consequently, it is important to study how organizational and environmental features of classrooms and the personal characteristics of students link to stabilize interracial friendship choices.

## PSYCHOLOGICAL PERSPECTIVES ON FRIENDSHIP STABILITY

There are a few psychological perspectives on friendship stability and dissolution (see Duck 1982 for a review of these perspectives). The simplest, and perhaps most naive, view is that friendship dissolution is the inverse of friendship formation. That is, factors that affect the establishment of friendships, such as similarity, also explain their dissolution. Duck and Allison (1978) show that pairs who formed friendships had more similar personality characteristics than those who did not; moreover, friendships that dissolved had more dissimilar members than those that persisted.

Another viewpoint emphasizes the importance of communication within a friendship dyad. Inadequate interactions between friends lead to their break up. Perhaps those with poor communication skills are unable to reveal their common characteristics, even where they exist. The cause of poor interactions may not stem from dyad members' characteristics, but from inadequate communication.

A third psychological perspective on friendship dissolution explains that one or both members of the dyad obtain new information about the other that damages the relationship. In this view, the factors dissolving friendships bear little relationship to those that build friendships.

While these perspectives seem plausible, they have limited application. By failing to specify either the relation between the individual

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qualities of the dyad members or the types of friendship interactions, the perspectives cannot explain or predict. More importantly, they fail to consider the context of friendships and, consequently, ignore the contribution of environmental factors to the stability of interpersonal relationships. Clearly, sociological analysis must supplement psychological theories of friendship dissolution. Our paper does this.

#### ORGANIZATIONAL AND ENVIRONMENTAL EFFECTS ON FRIENDSHIP STABILITY

The duration of a friendship is determined by the strength of the bond between two persons. Levinger (1976) calls the strength of this bond *cohesiveness*. He argues that three components influence the cohesiveness of a social relationship: a) the attractiveness of the relationship itself; b) the attractiveness of actual or potential alternatives to the relationship; and c) the barriers that contain persons in the relationship. The more attractive a relationship, the fewer the actual or potential alternatives to the relationship, and the greater the barriers to dissolving the relationship, the more stable the relationship is likely to be.

Most research on relationship dissolution has focused on marital dissolution (Duck 1982; Duck and Gilmour 1981) and has concentrated primarily on the interpersonal attraction component of cohesiveness. This research views relationship dissolution as the inverse of relationship formation. Yet attraction is only one of several important components that affect cohesiveness.

We argue that organizational and environmental factors affect the stability of friendships by influencing the three components of cohesiveness. First, change in interpersonal attraction is caused by changes in propinquity, personal similarity, and prestige or status. Propinquity is a necessary, but not a sufficient, condition for interpersonal attraction. Contact is needed, especially among youth, to provide the rewards of social relationships. Similarity has a dynamic relationship to interpersonal attraction. Once a friendship has been established, interaction between dyad members either creates new similarities or produces an awareness of previously unnoticed or irrelevant dissimilarities. When dyad members become more similar over time, interpersonal attraction increases; in contrast, dissimilarity reduces attraction. Finally, an individual's status among peers may change over time. If the status of both members of a friendship dyad increases, interpersonal attraction is likely to stabilize or increase. But if status changes create too much discrepancy

between the dyad members, their attraction likely decreases.

The major classroom characteristics that affect the bases of interpersonal attraction are the organization of instruction, classroom racial composition, and classroom climate. Each of these characteristics affects one or more of the bases of interpersonal attraction. The assignment of students to grades, classrooms, and instructional groups affects pupil propinquity. The racial composition of a class stimulates or limits opportunities for interracial interaction. Assigning students to the same instructional unit exposes them to similar educational activities and experiences that produce new similarities. The racial composition of the class affects student opportunities to transcend racial differences and focus on existing similarities. Classroom climate, which reflects teachers' values and attitudes, affects how students evaluate their similarities, such as academic performance, for their friendships. Finally, organizational and contextual features of a classroom affect student status. Assigning students to tracks or ability groups locates them on a visible status hierarchy; classroom climate stresses certain status characteristics and devalues others. By affecting each base of interpersonal attraction, classroom characteristics influence the first, and possibly most important, component of friendship cohesiveness.

The second component of cohesiveness, the attractiveness of actual or potential alternatives to a relationship, is also influenced by classroom factors. Interracial friendships are expected to be less stable in classrooms where a large number of same-race friends are available because same-race peers compete for friendship choices. The way instruction is organized creates opportunities for students to interact with peers they may not have sought out otherwise. These interactions may lead to new friendships that are more rewarding than existing ones. Classroom climate underscores some pupil characteristics that students might otherwise have ignored, opening up new possibilities for friendships that might not have occurred in another environment.

Finally, organizational and environmental features of a classroom affect the third component of cohesiveness, barriers to terminating friendships. Classroom racial composition may offer students in the numerical minority who are reluctant to make interracial friendships few opportunities for friendships. This situation would prevent the termination of existing friendships since few alternatives are available. Where classroom social climate supports existing friendships, peer sanctions against terminating a friendship could stabilize friendships. Similarly, when friendships emerge among

students assigned to the same instructional groups, the required work-related contacts may lead them to view friendship dissolution as too costly.

Characteristics of individual students, such as sex, achievement level, and reciprocity, will likely influence the duration of interracial friendships. Because similarity is a basis of interpersonal attraction, interracial friendships of the same sex will likely endure more than interracial friendships between boys and girls. Students tend to admire their successful peers, so they may prefer friends whose class rank is higher than their own. Hence, achievement difference may positively affect the stability of the friendship choices of lower-ranking students. This suggests that the interracial friendship choices of blacks may be more stable than those of whites, since whites typically rank higher in achievement. Since reciprocated choices are more rewarding than unreciprocated ones (Gouldner 1960), interracial friendships are likely to be more stable where both members of the dyad regard the other as a friend.

This paper has two aims: (1) to determine the length of interracial friendships for a sample of students in desegregated classrooms and to compare their stability to same-race friendships; (2) to examine the effects of classroom organizational characteristics and individual student characteristics on the stability of interracial and same-race friendships. This inferential analysis will show whether contextual factors that have been ignored in psychological research on friendship dissolution do, as we have argued, affect the stability of students' interracial friendships. It will also reveal whether factors that promote the dissolution of interracial friendships have a similar impact on same-race ones.

## METHODOLOGY

### *Sample*

In 1976-77, a large, longitudinal data set was obtained from 1,477 students in 48 classes in six public and four private schools in northern California. The data examined determinants of students' social relationships and academic achievement. The schools and classes were selected to represent varying organizational characteristics and racial compositions. Sixteen classes in this sample were desegregated and contained at least three students of a racial minority, whether black or white. The 455 students in these classes comprise the sample. The classes included 4 fourth grades, 4 fifth grades, 4 sixth grades, 3 seventh grades, and one sixth-seventh grade combination.

The sample contains 229 black students and 226 non-black students. The latter, referred to as

white hereafter, include a few Asian and Chicano students but not enough to create a separate category for analysis. The white students, on the average, came from higher socioeconomic backgrounds than their black classmates. The classes were fairly evenly divided between majority black and majority white, with three classes approximately racially balanced (between 40 percent and 60 percent black).

Not all the students received parental permission to participate in the study and some chose not to take part. In addition, some students were absent from class on the days the friendship information was collected. While efforts were made to obtain these data when the child returned to school, this was not always possible. As a result of these factors, the final sample contained complete information on 375 of the 455 students (82 percent) in the 16 classes. Classroom observation revealed no systematic differences in the social interactions of the students who were included in the sample and those who were not.

The students were given a sociometric questionnaire six times during the school year at approximately six-week intervals. The first data collection was scheduled during the first two weeks of the academic year. The students were given a list of their classmates and, next to each name, were the categories: "Best Friend", "Friend", "Know", "Don't Know", and "My Name". They were asked to circle the appropriate category for each student and encouraged to name as many best friends and friends as they wished. They were also told it was not necessary to name any friends if their friends were in a different class or school.

In addition to the friendship data, information on the students' background and achievement was obtained from school records. Standardized achievement test scores in reading were recorded for all the students. When different standardized tests were used, the scores were changed to the same metric using the transformations of the Anchor test study (Loret 1974). Information was also obtained from the teachers about the classroom climate and about their pedagogical practices, including the assignment of students to instructional groups.

To examine the determinants of interracial friendship stability, a dyadic-level analysis is required. In each dyad, *P* is designated the chooser and *O* the student who can be chosen. We examine those dyads in which *P* chooses *O* as Best Friend at some time during the course of the school year. Our interest is the stability of that choice. The dependent variable for the descriptive analysis in Table 2 is the termination of *P*'s choice of *O* (Dissol), coded as unity if the friendship dissolved and zero if the friendship

continued. The dependent variable is the same for the inferential analyses reported in Tables 3, 4, and 5, except that coding is reversed (1 = continuation, 0 = dissolution) to facilitate interpretation of parameter estimates. The best friend choices are used instead of the weaker friend choices because the latter are likely to contain more response error.

The independent variables include organizational-, dyadic-, and individual-level variables. The organizational variables are PROPORTION BLACK (PROP BLACK), CLASSROOM CLIMATE (CLIMATE), CLASS SIZE, and GRADE. The climate variable measures teacher emphasis on objective measures of academic achievement. It was obtained by factor-analyzing questionnaire data provided by the teachers. The items that loaded on the climate factor are emphasis on good grades, mastery of the curriculum, and basic skills. A low score implies little emphasis on these items. Class size is a control variable because the majority black classes tended to be larger than the majority white ones. Grade is as much a developmental factor as an organizational one and represents a control for student age or level of maturity.

Three dyadic variables pertaining to ability group membership are included: whether or not the dyad members are in the same reading group (READ-SAME), different groups (READ-DIFF), or in ungrouped classes (READ-UNGR). These dichotomous variables are coded as unity if the dyad members are in the same reading group, in different groups, or in ungrouped classes, and zero otherwise.

Other dyadic and individual level variables are: sex of *P* (SEX-P), coded as unity for female and zero for male; whether the members of the dyad are the same sex (SAMESEX), coded unity if yes and zero otherwise; reciprocity (RECIP), or whether *O* chooses *P* as Best Friend at the time period of interest, coded unity if yes and zero otherwise; and difference in rank in achievement (RANK DIFF), measured as the difference between *P*'s and *O*'s rank in class, based on the students' scores on a standardized achievement test in reading.

The variable old friends (OLD FRNDS) distinguishes friendship choices formed prior to the school year (or within the first two weeks of school) from those choices formed during the year. It is coded as unity if the friendship existed at the first time data were collected and zero otherwise. As noted below, each time period that a friendship choice is at risk of dissolution is treated as a separate observation or unit of analysis. To see whether friendship choices are especially unstable shortly after their formation, the variable, PERIOD 1, is included. It is coded as unity if the record is from the first time period

the friendship choice is at risk of dissolution and zero otherwise.

### *Procedures*

To obtain the dyadic-level data file for the analysis, records were created for all possible dyadic combinations of students within each of the 16 classrooms. Each dyad is included in the sample twice; in the first case, one member of the dyad is designated as *P*, the chooser, and the other member as *O*, the person chosen. In the second case, the chooser and chosen designation is reversed. This redundancy is necessary because friendship choices need not be mutual. To prevent standard errors from being inflated, each dyad is weighted by one-half in the inferential analysis.

Of the almost 13,000 dyads in the sample, 3,103 dyads were identified in which one student named the other as best friend at some point in time before the end of the school year. An additional 519 friendship choices that were not made until the last observational period were excluded from the analysis. Since these late-forming friendship choices were not at risk of dissolving until after data collection had been completed, nothing is known about their stability. There were 586 dyads in which a black student chose a white peer as best friend, 366 in which a white chose a black as best friend, 1,358 in which a black chose a black as best friend, and 793 in which a white chose a white as best friend. The same-race friendship choices are included in the analysis as a baseline against which to interpret the stability of interracial friendship choices and the factors that affect it. The duration of these friendships is the subject of interest here.

Analyzing the stability of dyadic friendship choices is not straightforward. It is tempting to do a conventional regression analysis in which the observed duration of the friendship is the dependent variable. However, Allison (1984) has outlined a number of reasons why such a strategy is inappropriate for individual-level data. The basic problems are the same for dyadic-level data.

First, the ultimate duration of a friendship choice is not known for choices that were still in existence at the end of the school year. These observations are said to be "right-censored." Simply using the observed duration clearly underestimates the true duration and can produce substantial biases. Further, it has been shown that excluding the censored observations is also highly problematic (Sørensen 1977; Tuma and Hannan 1978).

Second, even during the school year, it is not known exactly when the friendship choices began or ended. Only the status of the friendship at each of the six observational periods is



known. Assumptions of methods that require precise interval-level measurement may be violated.

Third, the values of some explanatory variables of interest can change across time (e.g., whether or not both members of the dyad are in the same reading group, or whether or not friendship choices are reciprocated). Changes in the values of variables might affect the stability of the friendship choice. Conventional regression techniques do not provide any convenient means of incorporating time-varying explanatory variables in the analysis.

Finally, many of the dyads are not only right-censored, but left-censored as well. Over half of the friendship choices already existed by the first observational period. These choices were made either extremely early in the school year or before school begun, but it is impossible to tell exactly when. Thus, again, the true value of duration is not known. Further, it seems reasonable to suspect that friendship choices made prior to the school year may differ substantially from those formed during it.

Allison (1982, 1984) has proposed a technique for dealing with the first three of these problems. The strategy treats each discrete time unit for each dyad as a separate observation or unit of analysis. If the friendship choice ended after four time periods, four different observations would be created. On the first three observations, dissolution would be coded 0 while on the last observation it would be coded unity. Time periods in which the friendship choice did not yet exist, was just being reported for the first time, or after the friendship choice had already terminated, are excluded from the analysis because the friendship choice was not at risk of dissolving at those times. Explanatory variables for each of these new observations are assigned whatever values they had at that particular unit of time. The final step is to pool the observations and compute maximum likelihood estimates for the logistic regression model.

Allison's technique addresses each of the first three concerns we presented. Dyads in which duration of a friendship choice is censored contribute exactly what is known about them—that the friendship choice did not end in any of the time periods in which they were observed. The method does not require that the duration be precisely measured; simply knowing the status of the friendship choice at each of the different observational periods is sufficient. Time-varying explanatory variables are easily incorporated into the analysis because each six-week interval the friendship choice is at risk is treated as a distinct observation.

The final problem of left-censoring is not so easily dealt with. One approach is to simply discard the initially censored intervals (Allison

1984). However, an examination of differences between friendship choices formed before the school year and those formed during it may be of interest. Therefore, we perform analyses on the total sample and separate analyses for the left-censored and non-left-censored observations.

Using Allison's approach, 9,286 records can be created from the 3,103 dyads in the total sample. Of these, 3,040 records are obtained from the 1,412 dyads in which *P* first chooses *O* as Best Friend at some point during the school year (the friendship choices that are not left-censored), and 6,246 records are created from the 1,692 dyads in which *P* chose *O* as Best Friend at some point prior to the first observational period (the left-censored cases).

Since there are only two possible outcomes for each friendship choice (continuation or dissolution), we analyze the data using a logistic regression model. Specifically, we estimate a model of the form:  $\text{Pr}(\text{frndshp choice continues})/\text{Pr}(\text{frndshp choice dissolves}) =$

$$\exp(\alpha + \sum \beta_j X_j).$$

A positive beta coefficient  $\beta_j$  implies that the friendship choice dyads that have a higher value on the independent variable *X* will tend to survive longer, while a negative coefficient implies that a higher value on the independent variable will lead to shorter friendship choices.

## RESULTS

### *Descriptive Analyses*

The means and standard deviations of the independent variables in the analysis are presented in Table 1 for the total sample and broken down by the race of each dyad type. The black-white dyads are those in which the chooser, *P*, is a black student and the chosen, *O*, is a white student, while the white-black dyads are those in which *P* is white and *O* is black. The left-censored and uncensored dyads are pooled in Table 1 because most of the differences in the means for the two samples are small. A few noteworthy exceptions are pointed out below.

The independent variables were measured at the beginning of the school year except for the time-varying variables (RECIP, READ-SAME, READ-UNGR, READ-DIFF), which were measured at each time period the friendship choice was at risk. The reading group measures varied little in value over time, so only their means for the first period at risk are presented in Table 1. The mean for reciprocation is given both for the first time interval and over all intervals.

Table 1. Means and Standard Deviations of Organizational, Dyadic, and Individual Level Variables—Full Sample

Variable	Total (N = 3,103)	Bl-Wh (N = 586)	Wh-Bl (N = 366)	Bl-Bl (N = 1,358)	Wh-Wh (N = 793)
Recip(1) <sup>a</sup>	.41	.25	.41	.44	.47
( $\alpha = .000$ )	(.49)	(.44)	(.49)	(.50)	(.50)
Recip(k) <sup>a</sup>	.47	.32	.49	.48	.54
( $\alpha = .000$ )	(.50)	(.47)	(.50)	(.50)	(.50)
Sex-P	.44	.53	.44	.41	.43
( $\alpha = .008$ )	(.50)	(.50)	(.50)	(.49)	(.50)
Same-sex	.79	.76	.85	.73	.90
( $\alpha = .000$ )	(.40)	(.43)	(.36)	(.44)	(.30)
Rankdiff	.86	7.81	-5.49	.01	.09
( $\alpha = .000$ )	(10.93)	(9.74)	(9.92)	(10.59)	(10.21)
Grade	5.27	4.96	5.07	5.58	5.07
( $\alpha = .000$ )	(1.10)	(.99)	(1.03)	(1.17)	(.96)
Classize	29.91	29.10	28.94	31.17	28.78
( $\alpha = .000$ )	(4.58)	(4.99)	(4.88)	(3.35)	(5.36)
Read-Ungr	.30	.27	.33	.38	.16
( $\alpha = .000$ )	(.46)	(.45)	(.47)	(.48)	(.37)
Read-Same	.24	.22	.25	.23	.28
( $\alpha = .196$ )	(.43)	(.41)	(.44)	(.42)	(.45)
Read-Diff	.46	.51	.42	.39	.56
( $\alpha = .000$ )	(.50)	(.50)	(.49)	(.49)	(.50)
Prop Black	.52	.44	.49	.70	.28
( $\alpha = .000$ )	(.26)	(.23)	(.23)	(.17)	(.15)
Climate	.41	-.11	-.02	.25	-.16
( $\alpha = .000$ )	(.85)	(.81)	(.80)	(.78)	(.94)
Period 1	.33	.35	.36	.32	.33
( $\alpha = .216$ )	(.47)	(.48)	(.48)	(.47)	(.47)
Old Frds	.55	.53	.41	.58	.57
( $\alpha = .000$ )	(.50)	(.50)	(.49)	(.49)	(.50)

Note: Standard deviations are in parentheses.

<sup>a</sup> Recip(1) refers to reciprocation during the first interval the dyad is at risk, while Recip(k) refers to the average reciprocation over all intervals.

Table 1 shows that, during the first time interval the friendship choice is at risk, *O* reciprocates *P*'s friendship choice (RECIP(1)) 41 percent of the time. For all time intervals taken together, the average rate of reciprocation (RECIP(K)) is 47 percent. Blacks who choose whites as friends are by far the least likely to have their choices reciprocated. Separate analyses (not shown) reveal that the reciprocation rate is 40 percent to 60 percent higher for the pre-existing friendship choices than for those formed during the school year. The one exception is the black-white dyads, in which the rate of reciprocation is about the same for the friendship choices made before and after school started.

The sample has slightly more females than males. The chooser is more likely to be female in black-white dyads and male in the other dyad types. About 80 percent of the Best Friend choices are between students of the same sex, but blacks are substantially more likely than whites to name friends of the opposite sex. White students rank considerably higher in academic achievement than their black peers. Therefore, the average differences in rank in achievement are large in interracial friendships and trivial in same-race friendships. About 24

percent of the Best Friend dyads are in the same ability group for reading, 46 percent are in different groups and 30 percent are in ungrouped classrooms. The classes, on the average, are slightly more than 50 percent black. Thirty-three percent of the records for the Best Friend dyads come from the first time interval the friendship choice is at risk (Period 1). About 55 percent of the friendship choices existed before the first data collection (Old Friends), with white-black friendships the least likely to have formed before the school year. *F*-tests were performed to determine whether differences in the means across the dyad types are statistically significant. The significance levels for these tests also are reported in Table 1. Most differences between groups are statistically significant.

The maximum number of weeks friendship choices could last if they were formed during the school year is 30. In the full sample, the Best Friend choices are observed to last, on the average, about three-and-a-half intervals or 21 weeks (data not shown). The non-left-censored dyads have a shorter observed duration (about 16 weeks) and the left-censored dyads a longer one (about 26 weeks). Thus friendship choices that began before or at the very beginning of the

school year have greater longevity than those formed during the school year.

Observed durations are affected by both left- and right-censoring, and by when the friendship choice was made during the school year. To provide a clearer picture of friendship survival, the means and standard deviations of the dependent variable, dissolution, are presented in Table 2 for the total sample and for the left-censored and non-left-censored subsamples. These statistics are also broken down for each dyad type by race. The statistics show that the dissolution rate of Best Friend choices is highest during the first time interval the friendship is at risk. Twenty-six percent of the Best Friend choices were withdrawn within six weeks after they are first observed. On the average, however, only 15 percent of the friendship choices dissolved during each six-week period of exposure to risk, implying that friendship choices that survive an initial high period of risk have a much greater likelihood of survival during the later time periods.

The means indicate that a large proportion of the same-race and interracial best friend choices made by the students in the sample last for at least six weeks. The friendship choices in same-race dyads are more likely to endure for more than a six-week period than are those in the interracial dyads. The friendship choices in black-white dyads have the greatest risk of terminating in any one period (19 percent) and those in white-white dyads the least likelihood (14.9 percent). Differences in survival rates

across dyad types are significant at the .08 level during the first time interval and at the .02 level across all the time intervals.

Friendship choices in left-censored dyads have a far lower risk of dissolving than those in non-left-censored dyads (by a factor of about 2.5). Thus friendship choices formed before or at the very beginning of the school year are much more stable than those formed during the school year. On the whole, 38 percent of the friendship choices made during the school year end within six weeks of their formation. Conversely, only 16 percent of the pre-existing friendship choices end within the first six weeks after they are initially observed.

The observed durations (not shown) indicate that the length of Best Friend choices across dyad types differ by only a few tenths of an observational period (the equivalent of one or two weeks). Even the means of the more appropriate dissolution measures in Table 2 reveal only small, though statistically significant, differences in stability. This may seem surprising, but these data are right-censored and cannot reveal the duration of friendship choices that continue to exist at the end of the school year. A great deal of difference could exist in these dyads as they respond to differential influences, such as residential patterns, outside the school environment.

#### *Logistic Regression Analysis*

The dependent variable in the multivariate, logistic regression model is *P*'s choice of *O* as

Table 2. Means and Standard Deviations of Friendship Dissolution

<i>A. Full sample</i>					
Variable	Total ( <i>N</i> = 3,103)	Bl-Wh ( <i>N</i> = 586)	Wh-Bl ( <i>N</i> = 366)	Bl-Bl ( <i>N</i> = 1,358)	Wh-Wh ( <i>N</i> = 793)
Dissol(1)	.26	.30	.30	.23	.25
( $\alpha$ = .079)	(.44)	(.46)	(.46)	(.42)	(.43)
Dissol(k)	.15	.19	.16	.15	.14
( $\alpha$ = .025)	(.36)	(.39)	(.37)	(.35)	(.35)
<i>B. Friendships formed during the school year (non-left-censored)</i>					
Variable	Total ( <i>N</i> = 1,412)	Bl-Wh ( <i>N</i> = 277)	Wh-Bl ( <i>N</i> = 217)	Bl-Bl ( <i>N</i> = 574)	Wh-Wh ( <i>N</i> = 344)
Dissol(1)	.38	.40	.38	.35	.40
( $\alpha$ = .573)	(.48)	(.49)	(.49)	(.48)	(.49)
Dissol(k)	.26	.30	.24	.25	.27
( $\alpha$ = .351)	(.44)	(.46)	(.43)	(.43)	(.44)
<i>C. Friendships formed prior to the school year (left-censored)</i>					
Variable	Total ( <i>N</i> = 1,691)	Bl-Wh ( <i>N</i> = 309)	Wh-Bl ( <i>N</i> = 149)	Bl-Bl ( <i>N</i> = 784)	Wh-Wh ( <i>N</i> = 449)
Dissol(1)	.16	.21	.18	.15	.13
( $\alpha$ = .221)	(.36)	(.41)	(.39)	(.36)	(.34)
Dissol(k)	.10	.13	.09	.10	.08
( $\alpha$ = .079)	(.30)	(.33)	(.29)	(.30)	(.28)

Note: Dissol(1) refers to friendship dissolution during the first interval the dyad is at risk, while Dissol(k) refers to average friendship dissolution over all intervals.

Best Friend or non-Best Friend. The independent variables are those that have an effect on the survival of a friendship choice. The independent variables READ UNGR and READ DIFF are excluded from the final model because they had no effect on friendship survival either in preliminary bivariate analyses or in the multivariate analysis. We also conducted analyses with mutual best friend choices as the dependent variable. Our present analyses, which include a control for reciprocation, produce virtually the same results.

Table 3 reports the parameter estimates for the full sample. Proportion black and class climate emerge as important determinants of the stability of interracial friendship choices. In the total sample, and for white choosers in the subsamples, proportion black has a significant positive effect on the stability of friendship choices, as predicted. The greater the ratio of black to white students in a class, the more stable are white interracial and same-race friendship choices. The results are in the opposite direction for black-white choices, although the estimates are not statistically significant.

Classroom climate has a negative effect on the stability of friendship choices in the full sample and for all four dyad types, although it is statistically significant only for black-white and black-black dyads. This finding indicates that the interracial friendship choices of blacks are more stable in classrooms where teachers

deemphasize objective measures of academic achievement, such as grades and success in mastering the curriculum. This is also true for the same-race choices of black students.

The impact of the remaining three organizational variables on the stability of friendship choices is minor. The effects of grade and assignment to the same reading group are insignificant for the total sample and for the four racial groups. Class size has a weak negative effect that is barely statistically significant for white-black dyads.

The two individual level characteristics that have the strongest effects on friendship choice are reciprocity and same gender. Table 3 shows that having a Best Friend choice reciprocated has a statistically significant positive effect on the stability of a friendship choice for all four dyad types. The same is true of choosing a peer of the same gender.

Sex of *P* and difference in rank in achievement have weak effects on stability. Except in black-white dyads, female friendship choices are less stable than those of males, although the gender effect is only statistically significant in black-black dyads. Difference in rank in achievement has a positive effect on the stability of black-white choices, which suggests that blacks are attracted to their higher achieving white peers. Finally, Period 1 has a strong negative effect on the stability of friendship choices in the total sample and for all four dyad types. This

Table 3. Multivariate Logistic Regression of Friendship Stability on Organizational and Dyadic-Level Variables for Full Sample

Variable	Total (N = 3,103)	Bl-Wh (N = 586)	Wh-Bl (N = 366)	Bl-Bl (N = 1,358)	Wh-Wh (N = 793)
Intercept	1.34** (.50)	.04 (1.23)	4.15* (1.75)	.71 (.83)	.75 (1.15)
Recip	1.01*** (.10)	.72** (.24)	1.14*** (.31)	.88*** (.14)	1.44*** (.21)
Sex-P	-.25** (.09)	.00 (.20)	-.52 (.30)	-.49*** (.13)	-.13 (.19)
Same sex	.80*** (.10)	1.05*** (.24)	.98** (.40)	.73** (.14)	.99*** (.29)
Rankdiff	.01 (.01)	.03** (.01)	.01 (.02)	-.00 (.01)	.01 (.01)
Grade	.02 (.06)	.21 (.15)	-.36 (.20)	.14 (.13)	.06 (.14)
Classsize	-.02 (.01)	-.01 (.02)	-.07* (.03)	.00 (.02)	-.02 (.02)
Read Same	.07 (.10)	-.01 (.24)	.49 (.34)	-.09 (.15)	.21 (.21)
Prop Black	.64*** (.19)	-.27 (.48)	1.89** (.65)	.05 (.44)	1.80** (.70)
Climate	-.31*** (.08)	-.53** (.18)	-.17 (.25)	-.51** (.18)	-.19 (.14)
Period 1	-1.02*** (.09)	-.92*** (.19)	-1.45*** (.28)	-.84*** (.13)	-1.14*** (.18)

Note: Standard errors are in parentheses.

\* Significant at the .05 level.

\*\* Significant at the .01 level.

\*\*\* Significant at the .001 level.

result, which was observed earlier in the descriptive analysis, indicates that the interracial and same-race friendship choices of all the students are at greater risk of dissolving shortly after they are first observed than later in the friendship.

Tables 4 and 5 present the multivariate models for the non-left-censored and left-censored dyads, respectively. Comparing the statistically significant effects of the independent variables for the total sample shows that proportion black, class size, and Period 1 have stronger effects on the stability of friendship choices formed during the school year (Table 4), while classroom climate, reciprocity, sex-P, and same-sex have stronger effects on the stability of friendship choices made at or before the beginning of the school year (Table 5). These differences are maintained across most of the dyad types. The effect of class size in both samples is small; the effect of Period 1 is stronger in the uncensored dyads because they have not been in existence as long as the censored friendships. It appears that individual and dyadic level variables have a stronger impact on the stability of pre-existing friendship choices than on choices made for the first time during the school year. Organizational characteristics affect the stability of friendship choices in both left-censored and non-left-censored dyads. Classroom climate has a stronger effect on the stability of pre-existing friendship

choices, while class racial composition has a greater influence on those made during the school year.

In general, the multivariate analysis shows that organizational as well as dyadic and individual level variables influence the stability of the interracial and same-race friendship choices of black and white students. Classroom racial composition and classroom climate, in particular, exert a significant influence on the stability of students' interracial friendship choices and, to a lesser degree, students' same-race friendship choices. The other organizational characteristics, assignment to the same reading group and class size, have little influence on the stability of either interracial or same-race friendship choices. Of the dyadic and individual level characteristics examined, reciprocation and same sex are of greatest importance to the survival of a cross-race friendship choice. The results show that while the dyadic and individual level variables are stronger determinants of the stability of interracial friendship choices, organizational factors are also important predictors of the duration of these choices.

## DISCUSSION

One might think that because students' interracial friendships are fairly uncommon, they are also unstable. Our research shows that this is not the case. Interracial friendship choices in the

Table 4. Multivariate Logistic Regression of Friendship Stability on Organizational and Dyadic-Level Variables for Friendships Formed during the School Year

Variable	Total (N = 1,412)	Bl-Wh (N = 277)	Wh-Bl (N = 217)	Bl-Bl (N = 574)	Wh-Wh (N = 344)
Intercept	2.07** (.73)	-.84 (1.69)	8.20*** (2.46)	1.43 (1.28)	.72 (1.80)
Recip	0.71*** (.14)	.47 (.34)	.72 (.42)	.67** (.21)	.96*** (.30)
Sex-P	-.19 (.13)	-.28 (.30)	-.20 (.39)	-.26 (.19)	-.12 (.28)
Same-sex	.53*** (.14)	.87** (.35)	.60 (.48)	.38 (.20)	.85* (.40)
Rankdiff	.01 (.09)	.03* (.02)	.02 (.02)	.01 (.01)	.00 (.13)
Grade	-.05 (.01)	.31 (.23)	-.79** (.29)	.12 (.19)	.07 (.21)
Classsize	-.04** (.02)	-.01 (.03)	-.12** (.05)	-.04 (.03)	-.03 (.03)
Read Same	.02 (.15)	.24 (.34)	.24 (.45)	-.18 (.23)	.05 (.30)
Prop Black	.88*** (.28)	-.30 (.72)	1.53 (.81)	.54 (.65)	1.50 (1.30)
Climate	-.25* (.11)	-.52* (.26)	.25 (.32)	-.54* (.26)	-.21 (.20)
Period 1	-1.05*** (.13)	-.80** (.28)	-1.44*** (.39)	-.88*** (.19)	-1.27*** (.27)

Note: Standard errors are in parentheses.

\* Significant at the .05 level.

\*\* Significant at the .01 level.

\*\*\* Significant at the .001 level.

Table 5. Multivariate Logistic Regression of Friendship Stability on Organizational and Dyadic-Level Variables for Friendships Formed Prior to the School Year

Variable	Total (N = 1,691)	Bl-Wh (N = 309)	Wh-Bl (N = 149)	Bl-Bl (N = 784)	Wh-Wh (N = 449)
Intercept	1.44* (.73)	.86 (1.90)	-2.02 (3.47)	1.24 (1.19)	.82 (1.57)
Recip	1.14*** (.14)	1.18** (.39)	1.58** (.53)	.95*** (.20)	1.62*** (.30)
Sex-P	-.29* (.13)	.23 (.29)	-1.00 (.53)	-.60** (.19)	-.21 (.28)
Same-sex	.87*** (.16)	1.09** (.37)	2.08** (.83)	.81*** (.22)	.97* (.43)
Rankdiff	-.00 (.04)	.02 (.02)	-.02 (.02)	-.02 (.01)	.01 (.01)
Grade	.06 (.09)	.14 (.21)	.12 (.34)	.06 (.18)	.06 (.20)
Classsize	-.01 (.02)	-.03 (.04)	-.01 (.06)	.01 (.03)	-.02 (.03)
Read Same	.05 (.15)	-.33 (.33)	.62 (.61)	-.04 (.21)	.36 (.31)
Prop Black	.47 (.28)	.02 (.68)	2.90** (1.17)	-.15 (.63)	1.67 (.93)
Climate	-.41*** (.12)	-.66* (.28)	-.90* (.45)	-.38 (.25)	-.26 (.21)
Period 1	-.71*** (.13)	-.70** (.27)	-.97* (.47)	-.59** (.19)	-.68** (.27)

Note: Standard errors are in parentheses.

\* Significant at the .05 level.

\*\* Significant at the .01 level.

\*\*\* Significant at the .001 level.

desegregated classrooms in our sample were fairly stable. While they generally did not last the entire school year, they did continue for several weeks and often months. Indeed, students' interracial friendship choices were almost as stable as their same-race choices. This surprising result may be because interracial friendships are unlikely in the first place and are made only if there is a strong attraction between a black and white student that then sustains the relationship over time.

Contrary to the impression left by some psychological models of friendship stability, our research demonstrates that friendship dissolution cannot be understood without taking into account the context of the relationship. Admittedly, personality characteristics, attitudinal measures, and the quality of interpersonal interactions are major determinants of friendship stability. But these variables operate within an organizational structure and environment. Organizational and contextual factors influence the attractiveness of a relationship, provide alternatives to an existing friendship, and affect the strength of the barriers to friendship dissolution. As a result, models of friendship dissolution that ignore the setting can only inadequately explain this important social process.

Our study shows that two classroom factors—racial composition of the class and class climate—have a pronounced impact on the stability of interracial friendship choices. Inter-

estingly, these variables do not affect blacks and whites in the same way. The racial composition of a class primarily affects the stability of white interracial friendship choices, with white choices being more stable in classes with a high proportion of black students. The effect of class racial composition on black interracial choices is negligible. Since class racial composition is related to opportunities for interaction, these findings suggest that one way to stabilize interracial friendship choices, at least for whites, is to create classrooms in which the racial composition, or within-class organization, provides many opportunities for students to choose compatible peers of a different race as friends.

Classroom climate affects the stability of the interracial friendship choices of blacks and, to a lesser degree, whites. Blacks are more likely to withdraw their interracial friendship choices in classrooms with a strong emphasis on successful academic performance. This same tendency is found for whites, especially for friendship choices made before or at the beginning of the school year. In classrooms where objective measures of academic success are given priority, academic status discrepancies between blacks and whites likely assume considerable importance and act as a deterrent to friendship stability. Consequently, if school personnel are interested in fostering stable friendships between black and white students, they may need to

create a classroom climate that provides opportunities for black students to be held in esteem by their white peers. Greater status equality along various dimensions should decrease the negative impact of academic differences on the interracial friendship choices of both black and white students. Programs that create a more uniform distribution of academic achievement across black and white students should also increase the stability of interracial friendships.

The differential impact of class racial composition and class climate on black and white friendships suggests that different processes govern the stability and dissolution of interracial friendship choices for black and white students. The interracial friendship choices of blacks respond more to a classroom climate that influences the status hierarchy of the class, while the interracial choices of white students respond more to organizational factors that affect opportunities for interracial interaction. These findings may indicate that the components of cohesiveness assume differential weights for black and white students in their ongoing friendships. Future research should address this issue to discover the reasons for these differences.

The dyadic level variables, primarily same sex and reciprocity, have stronger effects on the stability of interracial friendship choices than do the organizational or environmental variables. This is not surprising. Personal characteristics of dyad members are usually more easily observed than organizational factors during the everyday interactions of dyad members.

Finally, our study shows that friendships formed prior to the school year are more stable than those formed during it. This is probably because many of the weaker friendships formed prior to the school year dissolved by the time the school year began. However, even pre-existing friendship choices are vulnerable to the impact of classroom factors. The racial composition of a class and the class climate represent new environments that pre-existing friendship choices must adapt to, and one mode of adaptation is to terminate the friendship choice. It may be that the environmental factors that influenced the interracial friendship choice made before the school year started, such as neighborhood proximity or unavailability of other peers, disappear in the classroom environment and are replaced by contextual factors that no longer support the friendship choice.

This research has several policy implications. Clearly, dyadic-level characteristics have the strongest impact on the stability of interracial friendship choices. However, it is also clear that schools are not powerless in this area. If school personnel wish to support interracial sociability in desegregated schools, they should try to

provide a classroom environment that promotes stable interracial friendship choices. Our study shows that this can be done by paying attention to the racial composition of the class and to the class climate. The ratio of black to white students can afford opportunities for black and white students to interact with each other to foster positive sentiment between them. The classroom climate can decrease major status differences between black and white students by providing opportunities for all students to win the esteem of their peers. Thus, by manipulating the environmental and organizational factors that affect interpersonal attraction and the cohesiveness of relationships, school administrators and teachers can help sustain interracial friendship ties once they are made.

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# SCHOOL PERFORMANCE, STATUS RELATIONS, AND THE STRUCTURE OF SENTIMENT: BRINGING THE TEACHER BACK IN\*

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*Research has failed to support the assumption that the academic difficulties of many minority and low-SES youth are due to their "outsider" standing relative to the middle-class culture that dominates schools. This study suggests that this proposition exaggerates the cultural hegemony of educational operations. Data on children in the first grade of a large, socially heterogeneous urban public school system show that not all teachers are given to status-related biases. Rather, teachers' own social origins exercise a strong influence on how they react to the status attributes of their students. In particular, low-status and minority pupils experience their greatest difficulties in the classrooms of high-status teachers. They are evaluated by their teachers as less mature, their teachers hold lower performance expectations for them, and their teachers score exceptionally low on perceived-school-climate measures. Moreover, year-end marks and standardized-test scores of such pupils apparently are depressed by these indicators of pupil-teacher social distance and teacher disaffection. A model of pupil-teacher background congruence is proposed as an alternative to the cultural hegemony framework, and the implications of such fit for the interpersonal dynamics of the classroom are discussed.*

Although the academic difficulties of many minority and economically disadvantaged youngsters are often attributed to their "outsider status" in the middle-class school culture, evidence to support this proposition is thin. For example, differences in secondary-school performance by social background are trivial for youngsters of similar ability levels (Alexander and Eckland 1980; Rehberg and Rosenthal 1978; Williams 1976; Sewell and Hauser 1980). On other "attainment" measures (e.g., college attendance rates, enrollment in a college-bound high school program), minority youth often fare better than equally able whites (Alexander and Cook 1982; Alexander et al. 1982). And while youngsters from advantaged backgrounds fare well on such indicators of school success, the evidence credits this more to differences in family processes (e.g., high levels of parent support) than to school policy or practice.

Failure to find socioeconomic disadvantages

in school processes seems anomalous because schools allegedly function to reproduce the social order (Bowles and Gintis 1976; Bourdieu 1977). The literature on educational stratification identifies many points of contact between schooling processes and the broader stratification system (e.g., the sorting, selecting, and gatekeeping functions of schooling), and it would be surprising indeed if patterns of advantage and disadvantage in society were not also discernible in schools.

Why, then, do these patterns fail to appear in studies designed to detect them? For one reason, all the cited studies deal with students at the secondary and the postsecondary levels, and they may look too far down the road to detect processes that limit the prospects of minority and disadvantaged youngsters. The literature on teacher-expectancy effects, for example, indicates that teachers form impressions about students' potential in the very early grades, and that these impressions frequently are grounded in superficial or inappropriate cues—of style, dress, deportment, and language (Rist 1970; 1973). In line with the self-fulfilling prophecy, youngsters so singled out are stigmatized and suffer from being thought of by their teachers and their peers, and even themselves, as "losers." One result is that chronic underachievement starts very early.

School achievement trajectories take form

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very early and are highly stable over time (Alexander and Cook 1982; Entwisle and Hayduk 1982), so that if the advantages and disadvantages of social origins are "cemented" into early achievement patterns, the damage may be complete well before high school. The practice in studies at the *secondary level* of controlling for testing levels or other measures of competency to sort out "ascription" from "achievement" obscures precisely those aspects of process that are expressed in cognitive outcomes at the earlier grade levels.

Teacher-expectancy effects thus could account for the failure of research at the secondary and postsecondary levels to detect direct ascriptive disparities in performance outcomes. But the evidence is far from compelling. Although Rosenthal and Jacobson's (1968) original experiments of the so-called "Pygmalion effect" were greeted with enthusiasm, their methodology has been severely criticized, and efforts to replicate their findings have been disappointing (Elashoff and Snow 1971; Dusek 1975; Brophy and Good 1974). Moreover, research to distinguish achievement from ascription in the primary grades (Davis and Haller 1981; Leiter and Brown 1985) has met little more success than studies at the secondary level to detect socioeconomic "bias" in achievement processes. While ill-founded teacher expectancies may well deflect some disadvantaged youngsters from the educational successes they might otherwise experience, it seems unlikely that such effects are so pervasive or potent as to be the source of schools' supposed "middle-class bias."

Reproductionist thinking goes astray, we believe, in assuming that the social relations of schooling are governed by a pervasive cultural hegemony. Teaching, to be sure, is the quintessential "middle-class" occupation, and its professional ideology to develop character along with cognitive skills is strongly normative (Lortie 1975). But even if all the actors are reading from the same script, subtleties of interpretation matter a great deal (Sarbin and Allen 1968).

Reflecting on the poor showing of the teacher-expectancy hypothesis, Brophy and Good (1974) advance a typology of teacher-response styles. Their framework distinguishes among "proactive," "reactive," and "overreactive" teacher types, the last prone to inflexible, stereotyped thinking and likely to set in motion self-fulfilling prophecies. Brophy and Good say little about the origins of differences in teachers' styles, but the possibility of socioeconomic bias presents itself. The "match" or "mismatch" of student-teacher backgrounds may be important, especially in the early grades. The transition from home to school requires adjustments for all youngsters. For some,

however, the school collar will fit more comfortably, and family background no doubt affects the fit. The way teachers respond to their students during this settling-in period may determine how well adjustment stresses are managed; whether teachers are sympathetic or hostile, conscientious or lax, skillful or inept surely must matter.

Teachers are not automatons, and a perspective that strips them of their biographies, dispositions, and affective orientations misses much of what breathes life into classroom-interaction patterns. In recent years the dominant theories and research agendas of educational sociologists have focused on everything but the teacher. Subcultural values, school and classroom organization, clashing interests of parents and school systems, and children's developmental needs were supposed to provide sufficient keys to the educational process and to the effects of schools. Eclipsing the teacher, sociologists conceived of the classroom as the arena in which schooling took place. Interest centered on "pupil inputs" and "school outputs". If noticed, the teacher was conceptualized as a gatekeeper or credential carrier, but rarely as a distinct actor with personal feelings, emotions, and social history.<sup>1</sup>

Racial and socioeconomic background are integral to personal and social identity, just as much for teachers as for students. The influence of such factors is pervasive in a highly stratified social order. As generalized status attributes (Berger, Cohen, and Zelditch 1972) and as crude, but meaningful, measures of shared experience, they condition what transpires in many activities, including the interpersonal dynamics of the classroom (Brophy and Good 1974). All teachers would be considered middle class by virtue of their professional affiliation, and most no doubt would identify themselves as such. However, we would expect considerable diversity in teachers' social origins. Teaching has always been considered a respectable career for women from all backgrounds, and for minority women it has long been one of the few readily penetrable professions.

An extensive literature on personal and political values documents the lasting imprint of social origins, even among the highly mobile

<sup>1</sup> This neglect of teacher's individuality is hardly peculiar to the reproductionist literature. Levin (1980), for example, has been especially critical of what he calls the "Professional Paradigm" of teacher effectiveness that dominates the education literature. This approach defines "capability" in terms of training and experience to the near exclusion of all else. Perhaps this narrowing of the field explains why studies so framed have proven singularly unenlightening.

(e.g., Barber 1970; Lopreato 1967; Thompson 1971). We are interested here in how vestiges of their backgrounds temper teachers' dealings with their pupils. We know, for example, that teachers who feel a sense of commitment to minority and disadvantaged youth and who think well of their abilities are more successful in working with such youngsters (Smith 1972; St. John 1971). It seems reasonable that a shared identity/common background should foster that sort of commitment. For one thing, "misleading cues" (e.g., style of dress, deportment, or language usage) will not be misconstrued as fundamental failings by teachers whose own backgrounds make these cues familiar.

Teachers from high-status backgrounds, on the other hand, will be less familiar with, and perhaps less comfortable with, working-class surroundings and poverty. The same applies to white teachers dealing with poor black youngsters. The status-expectancy literature (Berger et al. 1972) indicates that status cues assume exaggerated importance in unfamiliar interpersonal situations. High-status and white teachers who are out of their element and lack common experience with their students may find it difficult to identify with them and, as a consequence, have difficulty working well with them. For example, in a direct experimental test of some of Berger et al.'s ideas, white middle-class adults could not raise expectations of black inner-city youngsters, although black adults could (Entwisle and Webster 1974). This is precisely the sort of situation that fosters stereotyped response patterns, which, in turn, form the foundation of negative teacher expectancies (or lead students to have negative expectations about teachers).

The following analyses examines how interactions among student race, student SES, teacher race, and teacher SES impinge on students' academic performance in first grade. By focusing on the first year of school, our analysis should capture the adverse effects of school or classroom process that might be obscured in higher grades owing to persistence of achievement patterns from one year to the next. Because of the relatively large social distance that separates high-status teachers from low-status students, the spread between blacks and whites and between relatively low and relatively high SES students on year-end performance should be largest in the classrooms of white teachers and of teachers from higher-status backgrounds.

We also will investigate whether the difficulties experienced by disadvantaged youngsters can be attributed to the "distancing" phenomena mentioned above. Data on three aspects of teacher affect/social cognition are available. If

status mismatch<sup>2</sup> complicates student-teacher relations in the ways anticipated, this would be reflected in teachers offering less positive assessments than parents of pupils' maturity levels, in lower performance expectations being held by teachers, and in less-positive teacher assessments of the school climate.<sup>3</sup>

School achievement is indexed by year-end marks in reading, math, and conduct, and by standardized-test scores in verbal and quantitative domains. As achievement measures, test scores are relatively objective, while teacher's marks are intrinsically more subjective. Marks, in general, are more public than test scores (children know their own, and frequently know others'), as are the samplings of behavior on which they are based. They are the major means by which teachers communicate performance evaluations to parents and students, and they are the centerpiece of a dossier that will follow the student throughout the school career, shaping the preconceptions of subsequent teachers and structuring opportunities and experiences (see Rist 1970 and Rosenthal and Jacobson 1968 on the persistence of Pygmalion effects across grade levels). It is likely that marks are much more implicated than test scores in the social-psychology of classroom dynamics, and this probably holds for all relevant actors—the pupils, their parents, and their teachers.

Our framework anticipates that school performance will be most adversely affected by status factors under conditions of high teacher-status/low pupil-status mismatch, that these adverse effects will result from student-teacher social distance and from negative teacher affect, and that their consequences should be more pronounced for teachers' marks than for test scores. Mismatches in the other direction (i.e., low-status teachers working with high-status pupils) should not be as problematic. First, teachers' social standing as middle-class educators should help offset whatever distancing tendencies might carry over from their own disadvantaged backgrounds. Second, the valuations attached to their mismatched students' status attributes are positive, not negative, and hence ought not to be especially detrimental. Lastly, their white and high-SES students probably are not as dependent as are

<sup>2</sup> See Epstein and McPartland (1977) and Epstein (1983) for another perspective on home-school fit and a review of the considerations that recommend such a focus. Their analysis examines decision-making styles in the two settings.

<sup>3</sup> The importance of a positive school atmosphere has been underscored in many studies using widely differing methodologies (Brookover et al. 1978; McDill and Rigsby 1973; Rutter et al. 1979).

disadvantaged students on the schools' resources for intellectual nurturance (Heyns 1978). Such youngsters are in a good position to ride out any difficulties associated with teacher background, while the minority and low-SES students in their classes will benefit from their teachers' understanding and sensitivity. In such settings, then, achievements should be nearer to parity than in those where student-teacher relations work to the advantage of advantaged students and to the disadvantage of disadvantaged ones.

## METHODS

### *The Sample*

The data for this analysis come from the Beginning School Study (BSS), a longitudinal study of youngsters attending first grade in Baltimore City elementary schools in the fall of 1982. Twenty schools were selected at random from within strata defined by racial composition and status background (i.e., blue collar versus white collar). To begin obtaining parental consent during the summer, kindergarten rosters from 1981-82 served as initial sampling lists. These were supplemented by rosters of new registrants in the fall. Both rosters were used to draw random samples of children from each first-grade classroom in the 20 schools in September 1982. Less than 3 percent of the sampled children had to be excluded because of parent refusals. These procedures yielded a final sample of 825 Baltimore City first graders, about equally divided by race and representative of all socioeconomic levels in the school system. Twenty-eight of the 825 were first grade repeaters. The results presented below are identical in all important respects when these youngsters are excluded from the analysis. The BSS is an ongoing panel, but this analysis relies almost exclusively on data from the first year of fieldwork.

Beginning in the summer and continuing into the fall of 1982, about 800 parents (usually the mother) were interviewed. In the summer following first grade, about 600 of these parents were re-interviewed. Pupils were interviewed individually on two occasions during first grade (although no data from these interviews are used in the present analysis), and first-grade teachers were asked to respond to three questionnaires, staggered throughout the school year. In all, 50 of 56 first-grade teachers provided some data. School marks and CAT scores were obtained from school records.

### *Student Variables*

The measures pertaining to pupil background and performance are described next. Measures

involving the teachers, their perceptions of the students, and their affective orientations are taken up in a separate section.<sup>4</sup>

*Race.* Race was coded 0 for white, 1 for black. The seven Oriental and Indian students in the sample were coded 0.

*Sex.* Sex was coded 0 for boys, 1 for girls.

*Par. Educ.* Information on parent's educational attainment, our measure of family SES, was obtained from the first parent interview. It measures the number of school years completed. For certain purposes the measure is trichotomized (i.e., less than high school, high school graduates, and schooling beyond high school).

### *Performance Outcomes*

*M, Marks.* Fourth-quarter marks in reading (R) and mathematics (M) are E (excellent), G (good), S (satisfactory), or U (unsatisfactory), coded from 4 to 1 respectively. Marks in conduct are coded 2 for satisfactory or 1 for needs improvement. First-quarter marks also are available and are used as controls in some of the analyses.

*CAT scores.* In October 1982 and May 1983, system-wide testing provided California Achievement Test scores (Level 11 Form C). The verbal CAT score used here is the average of four subtests (phonetic analysis, vocabulary, comprehension, and language). The math CAT score is the average of two subtests (computation and concepts). If one or more subtests was missing, the average is the average of the available subtests.

### *Teacher Variables*

All teachers in this sample are female, so teacher's gender is not considered.

*TRace.* Teacher's race is coded 0 for white, 1 for black.

*TSES.* Teacher's family origin status is coded in the SEI metric from information on her father's occupation when growing up.

<sup>4</sup> Our analysis uses data from school records, two parent interviews, and three teacher questionnaires. To avoid excessive sample loss owing to lack of response to various items or some gap in instrument coverage, missing values were imputed for some pupil variables—test scores, marks, parent's education, and parent's and teacher's maturity scale responses. About 15 percent of the values were missing on the measures procured from teachers and parents, 1-3 percent for the others. Complete data are available on pupil and teacher race. To improve the quality of the imputed values, means were computed separately for youngsters held back at the end of first grade and for those promoted. Missing values were assigned to individuals based on their own year-end promotion status. Missing data were not imputed for our measure of teacher's SES background, as the sample is subdivided on the basis of this variable throughout the analyses.

*Climate.* Classroom or school climate is measured by responses to three items repeated in the fall and spring teacher questionnaires. The scale score is the average of the teacher's responses to these six items. If either the fall or spring questionnaire was not returned, the average is based on the three items available. Response options ranged from 1 (most negative) to 5 (most positive). The items, with extremes in parentheses, are:

- a. For most faculty, teaching here is (very unpleasant; very pleasant).
- b. The climate in this school is (very tense; very warm).
- c. Trying to do your job right at this school is (very frustrating; very rewarding).

Coefficient alpha for the three-item fall scale is .89; for the three-item spring scale, .92. The fall-spring correlation is .74.

*Mature.* Teachers and parents provided separate evaluations of students' personal maturity by their responses to a series of 14 items taken from the 1976 version of the National Survey of Children. Using a grid labelled "exactly like", "very much like", "pretty much like", "somewhat like", "a little like", and "not at all like," teachers in March of 1983 rated each sample student in their classroom. Item values ranged from one to six, with scores ranging from less to more positive:

1. Very enthusiastic, interested in a lot of different things, likes to express his/her ideas.
2. Fights too much; teases, picks on or bullies other children.
3. Can't concentrate, can't pay attention for long.
4. Usually in a happy mood; very cheerful.
5. Rather high strung, tense, and nervous.
6. Is not liked much by other children.
7. Cheats; tells lies; is deceitful.
8. Shows creativity or originality in school work.
9. Acts too young for his/her age, cries a lot or has tantrums.
10. Has a very strong temper; loses it easily.
11. Is awfully restless, fidgets all the time, can't sit still.
12. Keeps to himself/herself; tends to withdraw.
13. Very timid, afraid of new things or new situations.
14. Is polite, helpful, considerate of others.

A perceived personal maturity score was derived by summing responses to these fourteen items. The alpha reliability of this scale is 0.87. (The alpha is reduced less than .02 when any single item is deleted.)

In the summer between the first and second

years, this same set of items was administered to 510 parents. This represents about 82 percent of the 625 panel youngsters who stayed in the original 20 schools through the second year. The others were lost owing to transfer, either outside the city system or to other schools within the city.<sup>5</sup> Coefficient alpha for the scale derived from the parents' responses is .74, considerably lower than that obtained for the teacher scale.

The simple difference in teacher and parent scale scores is used as a measure of differences in teacher and parent evaluations of student maturity. To measure perceived-maturity level, the teacher and parent responses are summed.

These constructions enable a sum and differences analysis of teacher-parent discrepancies analogous to the distinction between status-inconsistency effects and status-level effects in mobility studies (Hope 1975). Used together in a regression analysis, the effects of these two measures are equivalent to those that would be obtained as the main effects of the two separate scales. However, the level and difference expression of those effects is more appropriate to our interest in evaluating how differences in parents' and teachers' perceptions of the study children might influence school performance. The sum measure takes account of level of personal maturity as assessed by these two raters. With this effect controlled, coefficients associated with the difference construction reflect any consequences (or, more properly, correlates) of disparities in the valuations offered by parents and teachers. Suppose, for example, that a perception of exceptional maturity on the part of teachers impels youngsters toward high levels of accomplishment, as might be expected within a teacher-expectancy

<sup>5</sup> During the first two years of BSS fieldwork, youngsters were lost from the panel if they left the original set of 20 schools. By the end of first grade, about 120 of the original 825 had transferred out, and, by the beginning of the second grade, the sample size stood at 660. We subsequently have been able to expand our coverage to all public schools in the city system, and some of the youngsters lost during the first two years have been recovered. The present analysis, though, is based on those who remained in the schools originally sampled. Attrition during this period reveals no obvious biases. For example, 27 percent of the original sample consisted of black females, 28 percent of black males, 23 percent of white females and 22 percent of white males. The corresponding figures for the subsample of survivors through three years of fieldwork are 27, 29, 22 and 22, respectively. Similarly, the distribution of youngsters across school types (integration status by SES level) changes by no more than one percent across categories (e.g., the percent enrolled in black middle-class schools in the original sample was 12.0 percent; the figure after three years was 11.9 percent; for integrated middle-class schools the respective figures are 9.7 percent and 10.3 percent).

Table 1. Mean Values on Teachers' Attitude and Evaluation Measures

		Low-SES Teachers			High-SES Teachers			Low-SES Teachers		High-SES Teachers	
		Student SES			Student SES			Student Race		Student Race	
		<HS	HS	>HS	<HS	HS	>HS	White	Black	White	Black
Full Sample											
Climate	3.92 [.882]	3.97 (140)	4.12 (102)	4.11 (77)	3.55 (117)	3.89 (110)	3.94 (49)	3.93 (152)	4.15 (172)	4.39 (122)	3.27 (159)
Mature	1.29 [10.95]	2.09 (120)	1.36 (93)	3.26 (65)	-1.42 (102)	3.26 (104)	.178 (43)	1.52 (136)	2.60 (144)	2.87 (109)	-.659 (142)
T.Exp-R	2.35 [.932]	2.06 (136)	2.29 (95)	2.71 (65)	2.08 (113)	2.54 (106)	2.19 (47)	2.28 (149)	2.28 (151)	2.65 (118)	1.99 (153)
T.Exp-M	2.40 [.903]	2.16 (136)	2.43 (95)	2.85 (65)	2.15 (113)	2.52 (106)	2.22 (46)	2.49 (149)	2.31 (151)	2.68 (118)	2.01 (152)
T.Exp-C	1.87 [.332]	1.85 (135)	1.91 (94)	1.91 (65)	1.79 (113)	1.90 (106)	1.81 (47)	1.87 (147)	1.90 (151)	1.93 (118)	1.76 (153)
		White Teachers			Black Teachers			White Teachers		Black Teachers	
		Student SES			Student SES			Student Race		Student Race	
		<HS	HS	>HS	<HS	HS	>HS	White	Black	White	Black
Climate		3.93 (118)	3.86 (90)	4.30 (47)	3.68 (175)	4.00 (175)	4.04 (128)	4.13 (169)	3.65 (89)	4.04 (174)	3.82 (315)
Mature		1.24 (112)	.825 (81)	.408 (44)	.279 (155)	1.77 (178)	2.37 (140)	1.24 (156)	.585 (82)	2.55 (172)	.800 (304)
T.Exp-R		2.06 (113)	2.50 (88)	2.79 (42)	2.10 (166)	2.42 (163)	2.61 (125)	2.46 (160)	2.11 (85)	2.58 (176)	2.22 (286)
T.Exp-M		2.14 (113)	2.44 (88)	2.63 (43)	2.19 (166)	2.52 (162)	2.70 (125)	2.48 (161)	2.04 (85)	2.72 (176)	2.27 (285)
T.Exp-C		1.81 (114)	1.94 (88)	1.88 (43)	1.85 (166)	1.88 (160)	1.90 (126)	1.91 (162)	1.79 (85)	1.90 (174)	1.86 (286)

Note: Pooled sample standard deviations are presented in brackets. Sample sizes for particular mean values are presented in parentheses.

framework. With level of maturity controlled, higher teacher than parent assessments should be associated with more favorable outcomes. In a regression framework, this would be indicated by positive coefficients for the difference measure, net of any sum effects.

*T-Exp.* Early in the spring, teachers were asked their expectations of how well the sample students in their class would do in school *the following year*. These responses were procured after the third report card for the year but before the fourth, or final. Teacher expectations were obtained for reading, math, and conduct, with response options ranging from excellent to unsatisfactory in reading and math. For conduct, the options were satisfactory or needs improvement. The subject responses are coded from 4 to 1; conduct expectations are coded 2 and 1.

## RESULTS

Our framework directs attention to interactions between student and teacher-status attributes. The relevant descriptive comparisons are presented in Table 1. Mean values of the teacher perception/affect measures are displayed for the cross-classifications of teacher social origins, and of teacher race with student race and with student social background (as indexed by

parent's education). Two levels of teacher SES background are distinguished by dividing the teachers' distribution of father's SEI level at the sample mean (37 and below versus 38 and above). Some representative occupations at this status level would include police officer, restaurant or bar manager, jeweler or watchmaker, and farm manager—the "solid lower middle class." However, the sample-wide standard deviation is large (almost 23), implying that the teacher sample has been recruited from a wide range of family backgrounds. We also note that just over 70 percent of the youngsters in the BSS sample are taught by black teachers, whereas about 55 percent of the students in this sample are black.

Table 1 reveals that teachers' social origins and pupils' racial background have the most bearing on teachers' affective responses to their teaching situations and their perceptions/evaluations of their pupils. While differences associated with teachers' race and pupils' family status also are apparent, these, in general, are not as large or as consistently observed.<sup>6</sup>

<sup>6</sup> We should mention several sources of "slippage" in our implementation of the status-mismatch notion that might dampen such contrasts. Most important, different status dimensions have been used to locate parent's and

The top panel of Table 1 organizes these comparisons around the socioeconomic dimension of teacher background. The table entries are mean scores on the measures of teacher affect/evaluation for various student-teacher combinations. The first column gives the sample-wide mean. The figures in the next panel are for low-status origin teachers, computed separately for three student groupings: those whose parent respondent failed to complete high school; those whose parent respondent finished high school but had no postsecondary schooling; and those whose parent respondent had some formal schooling beyond high school. The third panel uses the same classification of student family status, but applies to teachers whose social origins are above the sample average for teachers. The last two panels also divide the teacher sample according to high versus low social origins, but classifies students according to race.

The first row of Table 1 pertains to the school-climate scale. The sample-wide average of 3.92 indicates a positive disposition overall, but there are differences across teachers, and these map onto student type differences in ways generally consistent with our expectations. For example, among low-SES teachers the spread in averages across student SES levels is .14 units, while the corresponding range among high-SES teachers is .39 units. The latter climate gap is about .44 climate scale standard deviations.

Similar differences are obtained when teachers' attitudes are evaluated separately for black and white youngsters. Among low-SES teachers, climate perceptions are similar regardless of pupil race. Among high-SES teachers, on the other hand, the two climate averages differ by more than a full standard deviation (1.12 units), favoring white pupils over black. The climate scores, it will be recalled, apply classroom-wide, while the table entries use the individual student as the unit of comparison. Given that

teacher's socioeconomic standing, and these don't map onto one another especially well. Unfortunately, it was not practical to array both parents and teachers along the same status dimension, as data on parent's occupation were not procured until the third year of BSS fieldwork, and our coverage is incomplete owing to further shrinkage in the sample size. Also, many of our respondents were unemployed or out of the workforce and reported no current occupation. Sample-size restrictions also limit us to a crude dichotomy in the case of teachers (while our tables refer to the teacher groupings as "high" versus "low" status, they actually are "above average" versus "below average"), and the case base is much smaller in critical cells than is desirable (e.g., the situation of high-SES teachers with high-SES pupils). For all these reasons we suspect our results provide a conservative picture of how status distinctions condition teachers' attitudes/evaluations.

there is some racial and socioeconomic heterogeneity in most of these settings,<sup>7</sup> this degree of differentiation at the individual level strikes us as quite impressive.

The next measure considered in Table 1 is the discrepancy between teacher and parent assessments of the study youngster's personal maturity. The sample-wide average of 1.29 indicates that teachers offered somewhat more positive evaluations overall, but, of course, this was not always the case.<sup>8</sup> The standard deviation for the distribution of difference scores, derived by subtracting parent's scores from teacher's, is just under 11 (values for the maturity scale can range from 14 to 84). Among low-SES teachers, we find positive difference averages throughout, with that for blacks exceeding whites and that for high-SES youngsters exceeding that for low. The differences, though, are all small. In comparison, differences involving high-SES teachers are much more substantial. In fact, for black youngsters and those from the lowest-SES families, teachers' evaluations average below those made by parents (this is indicated by negative table entries). The black-white difference is just over 3.5 points (or about .3 standard deviations), while the difference comparing mid-SES youth against low is just under five points (4.7), or over .4 standard deviations.<sup>9</sup>

The indicators of maturity used in this scale involve mainly "good pupil" and "receptive learner" role definitions (Kedar-Voivodas 1983), appropriate to the primary grades: is enthusiastic, doesn't tease, doesn't cheat, shows creativity, doesn't fidget, doesn't lose temper, and is polite and helpful. That high-SES teachers

<sup>7</sup> Six of the twenty schools are all (or nearly all) black and five are all (or nearly all) white. The percent black enrollment in the other nine ranges from 8 to 87, with five in the 25-65 range. In terms of parent's educational levels, the school averages range from 10.2 years to 15.7 years, with standard deviation ranging from 1.3 to 3.0.

<sup>8</sup> Although this is not reported in the table, it is worth noting that parent and teacher assessments generally corresponded (the zero order correlation between the two distributions was .40) and were similarly patterned. The sample-wide averages for teachers and parents are 68 and 67, respectively. The means are virtually identical for black students (67.5 versus 67.4) and differ by only 1.2 points for white students (with the teacher average being the higher of the two, at 68.6 versus 67.4). The averages increase in moving up the family SES gradient for both parent and teacher assessments and none of the average differences is large.

<sup>9</sup> Teachers' assessments of high-SES youth exceed parent evaluations by a smaller margin than for mid-SES youth, but the difference still is positive. The case base for the situation of high-SES teachers with high-SES students is in the low 40s, so it is not surprising that trends involving this group are less clear (and less secure) than others. This caution applies throughout these comparisons.

perceive their black students and their pupils from low-SES backgrounds as relatively lacking in such qualities does not augur well for developing a close bond between teacher and pupil.

High-SES teachers also hold lower expectations for their disadvantaged pupils' performance. Since expectations for performance in subject areas are measured on a four-point scale, and those for conduct on a two-point scale, small absolute differences can be large relatively, as we see in Table 1. The black-white differences of .66 units in reading, .67 in math, and .17 in conduct observed for high-SES teachers all are above .5 standard deviations, and all exceed by a considerable margin the corresponding differences observed for low-SES teachers. Comparisons across the student SES gradient do not conform to this pattern, however, due mainly to the relatively low expectations held by high-SES teachers for their high-SES youngsters. The case base here is quite small (only in the 40s) however, and may account for the blurring of trends.

The lower panel of Table 1 parallels the upper, except that teachers' race is substituted for teachers' SES background in constructing student-teacher groupings. While teachers' attitudes and evaluations tend to differ comparing high-SES against low-SES youngsters and comparing white students against black, the pattern of these differences is very similar regardless of teachers' race.<sup>10</sup> So, for example, the climate average for high-SES pupils is .4 units above that for low-SES pupils among both black and white teachers. The increase in performance expectations moving up the student SES gradient is also very similar among black and white teachers.

We have only considered simple descriptive comparisons thus far, and the picture could change considerably once the comparisons are adjusted for possible confounding factors (e.g., differences in student ability levels). Nevertheless, the patterns of these data allow several provisional conclusions. First, student race and student SES level both weigh upon teachers' attitudes and evaluations, and the differences associated with these aspects of student background are in some instances quite large. Second, high- and low-SES teachers react quite differently to these student characteristics. High-SES background teachers in particular are most sensitive to these details of student background. In contrast, teachers' race does not appear to interact with pupil background in

structuring teachers' attitudes and evaluations. In the analyses that follow, we examine whether this pattern holds when race and SES are considered jointly and when controls are introduced for differences in students' cognitive skills at the start of the school year.

Results from analysis of the teacher-affect measures are presented in Table 2, separately for four teacher groupings based on the cross-classification of race and SES origins (i.e., high- and low-SES blacks and whites.)<sup>11</sup> In the first row of results, each of the teacher-perception/affect variables is regressed against several student background predictors (student race, gender, and parent's educational level using the full range of values) and controls for fall CAT scores. When domain-specific teacher expectations are the outcomes, fall scores from that domain are used (i.e., verbal for reading expectations, quantitative for math expectations). For other outcomes, both verbal and quantitative CAT controls are employed. These fall testing data adjust for competency differences at the time of school entry, so effects of other student traits are estimated with these differences controlled.<sup>12</sup> Should racial and/or SES disparities persist, they presumably would have to be grounded in something other than competency differences, and the "status-

<sup>11</sup> Since we are interested mainly in interactions between teacher and pupil background, results for the full sample are not reported in favor of these subsample comparisons. The pooled results are available on request. The analyses reported in Tables 2, 3, and 4 are derived from pairwise data-present correlation matrices. The sample sizes differ substantially across teacher groupings, with that for low-SES white teachers being quite small (the number of cases having data on individual measures ranges from 51 to 44. The smallest case base for an element of the correlation matrix is 42). The corresponding figures for the high-SES white teacher group are 113, 102 and 93; for the low-SES black group, 273, 249 and 223; and for the high-SES black group 168, 140 and 134. Our main results—regarding the importance of teacher-SES/pupil-background interactions, the unimportance of teacher-race/pupil-background interactions, and the possible mediating role of the several affect/evaluation measures—all hold up when the teacher data are pooled across race (comparing high-SES against low-SES teachers) and across SES (comparing white teachers against black). The case base for these latter analyses are all larger than those reported here for the four teacher groupings. These tables too are available on request.

<sup>12</sup> These entry-level cognitive differences actually were quite small, at least along racial lines. Blacks scored only two points below whites in the verbal domain (or .06 pooled standard deviations), and five points in the math domain (or .22 pooled standard deviations). By year's end, though, the race gap had widened considerably in both domains (to 8.2 points and 10.1 points, respectively).

<sup>10</sup> The sole exception involves the climate measure—the margin of climate advantage accruing to whites is larger among the pupils of white teachers (.48 units, versus .22 for black teachers).



Table 2. Regression Predicting Teachers' Attitudes and Evaluations

D.V.	Low-SES White Teachers				Low-SES Black Teachers			
	Race	Par. Educ.	TSES	R <sup>2</sup>	Race	Par. Educ.	TSES	R <sup>2</sup>
Climate	.142 (.136)	.082 (.020)	—	.331 (.255)	.163* (.182)	.020 (.005)	—	.070 (.051)
	.018 (.018)	.011 (.003)	.428** (.087)	.409 (.326)	.163* (.182)	.020 (.005)	— .000 (— .000)	.070 (.047)
T.Exp-R	— .106 (— .137)	.041 (.014)	—	.153 (.075)	— .030 (— .054)	.116** (.044)	—	.311 (.299)
	.013 (.017)	.103 (.035)	— .391 (— .108)	.218 (.125)	— .023 (— .042)	.115 (.043)	— .025 (— .003)	.311 (.296)
T.Exp-M	— .055 (— .070)	— .156 (— .052)	—	.282 (.215)	— .085 (— .152)	.149* (.057)	—	.354 (.343)
	— .089 (— .113)	— .174 (— .058)	.080 (.022)	.286 (.201)	— .088 (— .159)	.149* (.057)	.013 (.002)	.354 (.340)
T.Exp-C	.041 (.029)	— .007 (— .001)	—	.101 (— .007)	.050 (.032)	.041 (.005)	—	.055 (.034)
	.121 (.087)	.039 (.007)	— .277 (— .042)	.133 (.006)	.056 (.035)	.039 (.005)	— .022 (— .001)	.055 (.030)
Mature	.296 (6.00)	— .514* (— 2.74)	—	.308 (.215)	.077 (1.62)	— .019 (— .085)	—	.057 (.036)
	.388** (7.87)	— .461* (— 2.45)	— .328 (— 1.41)	.357 (.228)	.033 (.696)	— .017 (— .076)	.121 (.190)	.072 (.043)
D.V.	High-SES White Teachers				High-SES Black Teachers			
	Race	Par. Educ.	TSES	R <sup>2</sup>	Race	Par. Educ.	TSES	R <sup>2</sup>
Climate	— .713* (— 1.35)	.062 (.033)	—	.544 (.520)	— .339* (— .925)	.263* (.174)	—	.263 (.238)
	— .714* (— 1.35)	.062 (.033)	— .003 (— .000)	.544 (.515)	— .576* (— 1.57)	.232* (.153)	— .537* (— .052)	.489 (.468)
T.Exp-R	— .194** (— .372)	.007 (.004)	—	.356 (.330)	— .383* (— .813)	.043 (.022)	—	.364 (.347)
	— .136 (— .261)	— .009 (— .005)	.206** (.016)	.395 (.363)	— .395* (— .839)	.041 (.021)	— .027 (— .002)	.365 (.343)
T.Exp-M	— .160 (— .310)	— .044 (— .024)	—	.359 (.333)	— .418* (— .853)	.000 (.000)	—	.453 (.439)
	— .064 (— .124)	— .078 (— .042)	.296* (.023)	.437 (.408)	— .413* (— .844)	.000 (.001)	.011 (.001)	.453 (.435)
T.Exp-C	— .264* (— .197)	.177 (.037)	—	.197 (.155)	— .162** (— .132)	.011 (.002)	—	.110 (.080)
	— .257* (— .191)	.175 (.036)	.024 (.001)	.198 (.147)	— .173 (— .140)	.010 (.002)	— .024 (— .001)	.110 (.074)
Mature	.136 (4.16)	— .045 (— .388)	—	.153 (.106)	— .269* (— 6.24)	.056 (.312)	—	.143 (.113)
	.204 (6.23)	— .074 (— .628)	.006 (.007)	.244 (.183)	— .283* (— 6.56)	.018 (.101)	— .178** (— .147)	.220 (.181)

Note: Metric coefficients are presented in parentheses. Coefficients for gender, fall CAT scores and, in the case of "Mature," the sum of parent and teacher maturity evaluations, are not reported. The second entry in the R<sup>2</sup> column is the level of explained variance after adjusting for degrees of freedom.

\* Significance at .01 level.

\*\* Significance at .05 level.

relations" accounting will have withstood this test of alternative possibilities.

The second row of results adds teachers' SES to the predictor set. This adjusts for possible main effects associated with the variability in status origins *within* our "high" and "low" groupings. To simplify the presentation, only coefficients for race and SES background are reported. Gender differences are small to inconsequential throughout, while the effects of fall test performance frequently are quite large.

The panels of Table 2 suggest that pupil-

teacher background interactions are indeed present, and that they revolve mainly around the teacher's SES origin-student's race nexus. For example, among low-SES teachers, both black and white, we find only one significant influence of student race on teacher affect/evaluations. This is for climate responses among low-SES black teachers, and it indicates somewhat more positive climate levels when teaching black students. On the other hand, among high-SES teachers, both black and white, eight of the ten coefficients involving pupil race are

significant. All of these favor whites over blacks, and some of them are strikingly large. The largest differences are sometimes found in the results for white teachers (i.e., school-climate scores), sometimes in the results for black teachers (i.e., performance expectations in subject areas and judgments relative to parent's of pupil maturity).

This pattern changes a bit when differences associated with the variability in teachers' SES *within groupings* are controlled in the second equation for each outcome. Some of the race main effects that are significant in the first row of results drop below the threshold of significance in the second, and the details differ for black and white teachers. Among white (high-SES) teachers, black pupils score below white only in terms of climate scores and conduct expectations, while among black (high-SES) teachers, all the race differences save that for conduct expectation remain significant. Although we are reluctant to make too much of such fine details, this pattern suggests that white teachers of minority youngsters focus on noncognitive considerations (e.g., deportment), while the "negativism" expressed by black teachers is more generalized.

In contrast to the rather striking differences involving student race, student social background exercises little independent influence on these measures for any teacher group (only a few such effects are significant and these are scattered). Scattered, too, are the significant coefficients obtained for the variability in teachers' SES origins that remains within the four teacher-type groupings.

Table 2, then, tends to confirm some of the impressions from Table 1, but other impressions do not hold up when controls for possible confounding factors are introduced (e.g., the differences associated with students' family SES). Most importantly, we find that pupil race strongly conditions the attitudes and evaluations of teachers from advantaged social origins, with black youngsters evoking more negative attitudes and poorer performance evaluations and possibly being judged less mature than their white peers. Among low-SES teachers, however, pupil race is practically irrelevant to such affective orientations and judgments. In Tables 3 and 4, we consider whether there are similar differences in year-end achievement levels across teacher groupings, and, if there are, whether these might be attributable to the differences in subjective orientations just documented. In Table 3, the performance criteria are report card marks. Test scores are evaluated in Table 4.

Again, separate regressions are performed for each of the four teacher-SES/teacher-race groupings. The first equation in each panel presents

the effects of pupil race and pupil family SES from analyses that include controls for gender and fall test scores. This parallels the procedures used in the first row of results from Table 2. The second row adds teacher SES, and in rows 3 through 5 one of the teacher-affect/perception measures is entered. By evaluating these measures separately, we ignore any redundancy in this predictor set. But here we are more interested in determining the plausibility of the congruence hypothesis than in trying to formulate a cogent model of teacher affect. For this agenda, the "wide-net" approach to rooting out possible sources of teacher influence seems warranted.

In several important respects, the trends in Tables 3 and 4 parallel those discussed above with respect to the affective/evaluative measures as outcomes. Particularly important is the pattern involving race differences in year-end achievements. Black performance falls short of white only in the classrooms of high-SES teachers: not a single race difference is significant among low-SES teachers. Among high-SES background teachers, on the other hand, most differences associated with student race are significant, and those involving marks as criteria generally are quite large. Importantly, this pattern holds equally as well among black teachers as among white.<sup>13</sup> It is teachers' status background rather than teachers' race that is the relevant line of demarcation.<sup>14</sup>

On the other side of the equation, it seems equally clear that student's race and not their SES level interacts with teachers' SES, both in deflecting achievement trajectories and, as we saw in Table 2, shaping teachers' affective responses. In fact, there are few significant differences associated with parents' educational level in any of these comparisons after differences associated with race, entry-level cognitive scores, and gender are taken into account.<sup>15</sup>

The panels of these tables also identify

<sup>13</sup> In fact, race differences in performance outcomes are more often significant among black teachers than white when TSES is controlled (see the verbal criteria—reading and CAT-V). Here again, though, we would not want to make too much of small differences at this level of detail.

<sup>14</sup> This conclusion is at odds with evidence produced by Murnane (1975, 73) that black teachers are more successful than white in teaching reading to inner-city black children. However, the confounding of teacher's race and SES in Murnane's study complicates direct comparisons with ours.

<sup>15</sup> There is a modest association between parents' education and fall CAT scores (.28 on the verbal subtest, .26 on the quantitative subtest, figures which compare favorably with national estimates; see Duncan 1968). It is these fall test controls that dampen the effects of parents' education on spring achievement levels.

Table 3. Regressions Predicting Fourth Quarter Marks

D.V.	Low-SES White Teachers					Low-SES Black Teachers				
	Race	Par. Educ.	TSES	"Affect"	R <sup>2</sup>	Race	Par. Educ.	TSES	"Affect"	R <sup>2</sup>
<i>Reading</i>										
	-.005 (-.006)	.020 (.006)	—	—	.071 (-.018)	-.089 (-.146)	.082 (.029)	—	—	.278 (.266)
	.119 (.131)	.085 (.025)	-.408 (-.095)	—	.141 (.036)	-.059 (-.097)	.076 (.026)	-.108 (-.013)	—	.289 (.274)
(Climate)	.136 (.150)	.080 (.023)	-.337 (-.079)	-.160 (-.185)	.157 (.031)	-.043 (-.070)	.076 (.026)	-.107 (-.013)	-.094 (-.138)	.297 (.279)
(Mature)	-.093 (-.103)	.228 (.066)	-.116 (-.027)	.452* (.025)	.503 (.404)	-.150* (-.246)	.021 (.007)	-.102** (-.012)	.173* (.013)	.486 (.470)
(T.Exp-R)	.107 (.119)	-.002 (-.000)	-.079 (-.019)	.839* (.713)	.691 (.643)	-.039 (-.065)	-.021 (-.007)	-.086* (-.011)	.846* (.778)	.782 (.776)
<i>Math</i>										
	.108 (.150)	-.063 (-.023)	—	—	.279 (.210)	-.095 (-.170)	.073 (.027)	—	—	.272 (.259)
	.129 (.179)	-.051 (-.019)	-.049 (-.014)	—	.280 (.192)	-.102 (-.181)	.074 (.028)	.024 (.003)	—	.272 (.257)
(Climate)	.126 (.175)	-.052 (-.019)	-.220 (-.065)	.281 (.407)	.330 (.229)	-.097 (-.173)	.075 (.028)	.024 (.003)	-.028 (-.045)	.273 (.255)
(Mature)	-.034 (-.047)	.015 (.005)	.108 (.032)	.216 (.015)	.486 (.383)	-.185* (-.330)	.041 (.015)	.013 (.002)	.197* (.017)	.402 (.383)
(T.Exp-M)	.193 (.267)	.072 (.026)	-.106 (-.031)	.713* (.774)	.643 (.588)	-.036 (-.064)	-.038 (-.014)	.014 (.002)	.746* (.740)	.632 (.622)
<i>Conduct</i>										
	-.243 (-.232)	.091 (.023)	—	—	.214 (.118)	.026 (.021)	.014 (.002)	—	—	.064 (.044)
	-.142 (-.135)	.149 (.037)	-.351 (-.071)	—	.266 (.156)	.036 (.029)	.011 (.002)	-.037 (-.002)	—	.065 (.041)
(Climate)	-.138 (-.132)	.151 (.038)	-.268 (-.054)	-.193 (-.193)	.288 (.161)	.054 (.044)	.014 (.002)	-.037 (-.002)	-.118 (-.085)	.078 (.051)
(Mature)	-.389** (-.372)	.294 (.074)	-.081 (-.016)	.400* (.019)	.598 (.500)	-.091 (-.074)	-.037 (-.006)	-.022 (-.001)	.106 (.004)	.314 (.289)
(T.Exp-C)	-.189 (-.181)	.134 (.034)	-.242 (-.049)	.393* (.524)	.400 (.287)	.006 (.005)	-.010 (-.002)	-.025 (-.002)	.531* (.684)	.332 (.311)

Table 3. Continued

D. V.	High-SES White Teachers					High-SES Black Teachers				
	Race	Par. Educ.	TSES	"Affect"	R <sup>2</sup>	Race	Par. Educ.	TSES	"Affect"	R <sup>2</sup>
<i>Reading</i>										
	-.203** (-.364)	.015 (.008)	—	—	.351 (.324)	-.361* (-.722)	.040 (.019)	—	—	.358 (.339)
	-.155 (-.278)	.001 (.001)	.170 (.012)	—	.378 (.344)	-.380* (-.760)	.037 (.018)	-.043 (-.003)	—	.360 (.336)
(Climate)	-.113 (-.203)	-.003 (-.002)	.171 (.012)	.057 (.054)	.379 (.339)	-.299* (-.559)	.005 (.002)	.032 (.002)	.139 (.103)	.370 (.342)
(Mature)	-.106 (-.190)	-.032 (-.016)	.095 (.007)	.186** (.011)	.567 (.532)	-.187** (-.375)	-.030 (-.015)	.050 (.004)	.089 (.008)	.569 (.545)
(T.Exp-R)	-.058 (-.104)	.008 (.004)	.024 (.002)	.710* (.666)	.683 (.662)	-.059 (-.118)	.004 (.002)	-.021 (-.001)	.813* (.766)	.780 (.770)
<i>Math</i>										
	-.308* (-.556)	.029 (.015)	—	—	.280 (.249)	-.315* (-.676)	.099 (.051)	—	—	.411 (.393)
	-.227** (-.410)	.000 (.000)	.249* (.018)	—	.335 (.299)	-.328* (-.705)	.097 (.050)	-.031 (-.002)	—	.412 (.390)
(Climate)	-.357* (-.645)	.011 (.006)	.248* (.018)	-.183 (-.175)	.350 (.308)	-.224** (-.482)	.055 (.029)	.062 (.005)	.172** (.136)	.427 (.402)
(Mature)	-.198** (-.359)	-.016 (-.008)	.174 (.012)	.121 (.007)	.443 (.398)	-.122 (-.262)	.039 (.020)	.057 (.004)	.115 (.010)	.600 (.578)
(T.Exp-M)	-.186** (-.337)	.050 (.025)	.060 (.004)	.638* (.595)	.564 (.536)	.003 (.007)	.096** (.050)	-.040 (-.003)	.803* (.845)	.764 (.753)
<i>Conduct</i>										
	-.309* (-.236)	.101 (.022)	—	—	.227 (.186)	-.354* (-.327)	-.019 (-.004)	—	—	.166 (.135)
	-.344* (-.263)	.113 (.024)	-.110 (-.003)	—	.238 (.188)	-.411* (-.379)	-.026 (-.006)	-.128 (-.004)	—	.179 (.142)
(Climate)	-.210 (-.161)	.102 (.022)	.110 (-.003)	.188 (.076)	.254 (.197)	-.487* (-.449)	.004 (.001)	-.199 (-.007)	-.132 (-.045)	.188 (.144)
(Mature)	-.368* (-.282)	.118 (.025)	-.180 (-.005)	.337* (.008)	.411 (.355)	-.259* (-.239)	-.065 (-.015)	-.058 (-.002)	.138 (.006)	.282 (.236)
(T.Exp-C)	-.185** (-.142)	.005 (.001)	-.125 (-.004)	.619* (.636)	.545 (.510)	-.030 (-.317)	-.030 (-.007)	-.119 (-.004)	.389 (.442)	.313 (.276)

Note: Metric coefficients are presented in parentheses. Coefficients for gender and fall CAT scores are not reported. The second entry in the R<sup>2</sup> column is the level of explained variance after adjusting for degrees of freedom. The "affect" measure included in a particular equation is identified in parentheses in the first column. When "Mature" is added as the "Affect" control, the measure constructed as the sum of parent and teacher maturity judgments also is included in the equation.

\* Significance at .01 level.

\*\* Significance at .05 level.

Table 4. Regressions Predicting Spring Test Performance

D.V.	Low-SES White Teachers					Low-SES Black Teachers				
	Race	Par. Educ.	TSES	"Affect"	R <sup>2</sup>	Race	Par. Educ.	TSES	"Affect"	R <sup>2</sup>
CAT-V										
	-.088 (-5.29)	.221 (3.47)	—	—	.178 (.105)	.047 (3.44)	.057 (.889)	—	—	.409 (.399)
	-.031 (-1.84)	.251 (3.95)	-.189 (-2.41)	—	.193 (.102)	.030 (2.22)	.061 (.947)	.060 (.331)	—	.413 (.400)
(Climate)	-.004 (-.229)	.242 (3.815)	-.080 (-1.023)	-.246 (-15.450)	.232 (.124)	.009 (.683)	.060 (.936)	.060 (.328)	.121* (7.95)	.426 (.412)
(Mature)	-.249 (-14.92)	.442* (6.97)	.084 (1.06)	.525* (1.555)	.485 (.382)	-.036 (-2.61)	.022 (.337)	.063 (.343)	.140* (4.88)	.519 (.504)
(T.Exp-R)	-.039 (-2.34)	.187 (2.94)	.052 (.665)	.618* (28.550)	.491 (.417)	.042 (3.07)	.005 (.074)	.073 (.398)	.492* (20.3)	.579 (.568)
CAT-Q										
	-.129 (-6.94)	-.028 (-.400)	—	—	.352 (.294)	-.015 (-.942)	.071 (.932)	—	—	.456 (.447)
	.013 (.685)	.047 (.659)	-.333** (-3.81)	—	.425 (.359)	-.011 (-.711)	.070 (.926)	-.013 (-.062)	—	.456 (.444)
(Climate)	.013 (.698)	.047 (.660)	-.318 (-3.64)	-.024 (-1.38)	.425 (.345)	-.014 (-.876)	.069 (.914)	-.014 (-.064)	.016 (.903)	.456 (.442)
(Mature)	-.159 (-8.57)	.155 (2.19)	-.162 (-1.85)	.290** (.769)	.575 (.490)	-.069 (-4.33)	.049 (.642)	-.029 (-1.34)	.197* (.585)	.534 (.519)
(T.Exp-M)	.047 (2.52)	.113 (1.60)	-.364* (-4.16)	.384* (16.2)	.550 (.461)	.022 (1.39)	.013 (.175)	-.018 (-.085)	.381* (13.2)	.549 (.538)

Table 4. Continued

D.V.	High-SES White Teachers					High-SES Black Teachers				
	Race	Par. Educ.	TSES	"Affect"	R <sup>2</sup>	Race	Par. Educ.	TSES	"Affect"	R <sup>2</sup>
CAT-V	-.191** (-14.3)	.099 (2.08)	—	—	.426 (.402)	-.101 (-7.72)	.102 (1.89)	—	—	.473 (.460)
	-.158 (-11.84)	.090 (1.88)	.116 (.343)	—	.438 (.408)	-.140** (-10.7)	.096 (1.78)	-.089 (-.243)	—	.480 (.463)
	-.003 (-.220)	.073 (1.54)	.119 (.352)	.211 (8.38)	.458 (.424)	-.076 (-5.85)	.071 (1.31)	-.030 (-.081)	.111 (3.11)	.486 (.465)
	-.149** (-11.18)	.072 (1.52)	.053 (.157)	.321* (7.88)	.631 (.601)	.011 (.861)	.040 (.744)	-.018 (-.050)	.034 (.113)	.626 (.607)
	-.092 (-6.92)	.094 (1.97)	.017 (.050)	.482* (18.9)	.578 (.551)	.084 (6.44)	.073 (1.35)	-.074 (-.201)	.569* (20.5)	.685 (.672)
CAT-Q	-.139 (-8.52)	.039 (.665)	—	—	.460 (.438)	-.217* (-13.6)	.140** (2.13)	—	—	.516 (.503)
	-.098 (-5.99)	.024 (.415)	.127 (.307)	—	.474 (.447)	-.283* (-17.8)	.130** (1.99)	-.152 (-.341)	—	.534 (.518)
	-.034 (-2.11)	.019 (.324)	.128 (.308)	.089 (2.89)	.478 (.445)	-.142 (-8.94)	.074 (1.13)	-.025 (-.056)	.233* (5.38)	.562 (.545)
	-.105 (-6.45)	.021 (.355)	.051 (.123)	.304* (6.10)	.641 (.612)	-.172* (-10.8)	.104 (1.58)	-.100 (-.224)	.124** (3.36)	.580 (.559)
	-.069 (-4.22)	.059 (1.02)	-.007 (-.017)	.453* (14.3)	.590 (.564)	-.138** (-8.69)	.130** (1.98)	-.156* (-.349)	.351* (10.8)	.601 (.585)

Note: Metric coefficients are presented in parentheses. Coefficients for gender and fall CAT scores are not reported. The second entry in the R<sup>2</sup> column is the level of explained variance after adjusting for degrees of freedom. The "affect" measure included in a particular equation is identified in parentheses in the first column. When "Mature" is added as the "Affect" control, the measure constructed as the sum of parent and teacher maturity judgments also is included in the equation.

\* Significance at .01 level.

\*\* Significance at .05 level.

teacher affect and evaluation as important influences on achievement outcomes. High levels of perceived maturity and high performance expectations, for example, are predictive of high marks and test scores in practically all situations, and several effects involving the school climate measure also are significant.<sup>16</sup>

These results, along with the suggestions that pupil-teacher interactions weigh more heavily on marks than on test scores as outcomes (compare race effects in the results for high-SES teachers across Tables 3 and 4), indicate that achievement levels, at least in first grade, are strongly influenced by the quality of student-teacher relationships and by the subjective/affective "spillover" from those relationships. What remains to be considered is whether such subjective/affective responses of teachers contribute to the substantial achievement disparities between blacks and whites in the classrooms of high-SES background teachers.

Evidence on this point is only suggestive, but the suggestions are consistent with the mismatch possibility. We find, for example, that when judgments of personal maturity are controlled (the fourth row of results for each outcome), the original race differences usually moderate substantially (the exceptions involve conduct marks as outcomes, which, of course, are not really cognitive indicators).<sup>17</sup> The attenuation of these race differences is even more pronounced when performance expectations are controlled, and, even though significant effects involving the school-climate measure are less numerous, these controls too shrink race differences in many instances.

It thus appears that low teacher expectations and unfavorable assessments of student maturity/immaturity could account for these race differences in school performance, which are observed only among high-SES background teachers. And while such perceptions/expectations are influential in all contexts, we saw earlier that high-status teachers held especially negative opinions of their minority students. As a general consideration, it probably is of some comfort to learn that teachers' ideas about their

students make a difference. As sources of encouragement, they no doubt impel many youngsters to higher levels of accomplishment than would be realized otherwise. But significant others can exercise their influence for good or bad, and, in the case of minority students of high-SES teachers, the net effect is decidedly negative.

## DISCUSSION

Studies of educational stratification (Hauser 1970) direct attention to ways in which the workings of schools perpetuate or moderate socioeconomic inequalities across generations. The evidence here indicates that not all teachers are equally given to status-related biases, for teachers' social origins exercise a strong influence on how they react to the status attributes of their pupils. Wilson's controversial thesis (1978) that class or socioeconomic distinctions have displaced race as an immediate factor in stratification processes receives some support in these data. Teachers' social origins, rather than their racial backgrounds, impair their effectiveness with certain kinds of youngsters, and black youngsters' school performance is most impaired. Although it is not obvious why this particular pupil-teacher combination stands out from among the several combinations considered, the pattern is certainly clear enough. Student's race, at least under certain conditions, remains highly resilient as a status attribute in conditioning the quality and character of student-teacher relations. Students' SES background, on the other hand, at least as operationalized via data on parent's education and in terms of the variability represented in this urban, northeast school system, has much less bearing on these interpersonal and achievement processes.

The evidence indicates that high-status teachers, both black and white, experience special difficulties relating to minority youngsters. They perceive such youngsters as relatively lacking in the qualities of personal maturity that make for a "good student," hold lower performance expectations of them, and evaluate the school climate much less favorably when working with such students. As a result, blacks who begin first grade with test scores very similar to their white age-mates have fallen noticeably behind by year's end. This probably is the onset of race-differentiated achievement trajectories, for we know that catching up, once having fallen behind, is exceedingly difficult. Our results suggest that the status dynamics of pupil-teacher relationships are very much implicated in this early shortfall.

Another issue needs comment: teacher gender. As most elementary teachers are female, particularly in first grade, these results are

<sup>16</sup> Conclusions here regarding teacher-expectation effects are altered very little even when first-quarter marks are used as additional controls. Although individual coefficients are in many instances substantially attenuated, all remain sizable and significant.

<sup>17</sup> These difference effects are estimated with level of personal maturity (i.e., the sum of teacher and parent responses) controlled. That maturity level seems to have very real consequences in terms of pupil achievement is indicated in the substantial increases in explained variance obtained when these measures are added to the regression model.

relevant to educational processes in many urban schools around the country. There are no male teachers in this BSS sample, but other research (Murnane 1975) suggests that male teachers are more successful in teaching reading and math, irrespective of student's sex. If teacher status is as critical for children's transition to first grade as the data in this paper suggest for other teacher-status attributes, the policy of assigning male teachers mainly to the upper elementary grades requires some review.

Rather remarkably, teachers' judgments and students' progress seem not to depend on the student's sex. This is surprising on two counts: 1) an extensive literature suggests elementary teachers "blame" boys (Brophy and Good 1974) and in other ways tend to interact with them differently from the ways they interact with girls. For example, girls get higher marks than their twin brothers (Doma cited by Finn 1972) and score higher on teachers' ratings of children's behavior in the first three grades (Alberti 1971). 2) Girls' achievement generally outpaces boys, but especially at the elementary level in reading (Averch et al. 1972). Other things equal, then, we would expect that teachers' perceptions of student's maturity would differ by sex, as would youngsters' gains on standardized tests.

There are few comprehensive data on first-grade achievement, but Entwisle and Hayduk (1982) observed sex differences in conduct marks, which also are indicated in our results (not reported in tables). Like the present study, however, they found no direct effects of sex on reading or math marks in first grade (or in grades two and three). To the extent that classroom deportment affects academic performance, however, there are indirect effects of deportment on marks.

The matter of gender aside, the present picture is sobering: when high social distance separates teacher from student, negative teacher perceptions, low expectations, and teacher disaffection ensue. And it is hardly surprising in such situations that teachers fail to bring out the best in their students. But the literature on status biases and teacher expectancies would have us believe that this rather bleak portrait of teacher-pupil relationships is pervasive. In failing to specify the conditions under which classroom dynamics might be expected to assume this form, previous studies have missed the mark. Teachers' reactions depend on their personal circumstances. Our model of teacher-pupil background "congruence" or "fit" suggested where lack of fit might be especially pernicious: the case of high-SES background teachers working with minority and disadvantaged youngsters.

Such status effects could be of great conse-

quence, even if they do not operate across the board. There is a perverse irony in the possibility that minority youngsters and those from disadvantaged backgrounds suffer academically because of their marginality relative to the dominant status culture. Many studies have shown the performance of minority and disadvantaged youngsters to be especially sensitive to the details of their school experience and to the characteristics of their teachers (Coleman et al. 1966; St. John 1971). Heyns' (1978) research shows that these are precisely the youth for whom schooling matters most. Hence, the consequences of such marginality are borne by those whose skill development is most dependent on the schools, and the impact is magnified owing to the receptiveness of such youth to school influence, be it for good or for bad.

As agents of academic socialization, teachers likely place second only to parents, and their influence probably is greatest in the primary grades, where youngsters are acclimating to the academic routine.<sup>18</sup> As the frontline representative of the school, the teacher mediates the student's relations to the broader institutional environment. Teachers embody organizational authority, and with young children they represent adult authority as well. In the classroom, the teacher doles out rewards and punishments, bears responsibility for performance evaluations, and maintains control over classroom resources. In the primary grades, the teacher also wipes runny noses and consoles hurt feelings, joining formal and nurturant responsibilities in a role peculiar to the elementary teacher.<sup>19</sup>

This blending of the instrumental and the affective precludes reducing the issue of teacher effectiveness to a tidy technical agenda or substituting the vague and impersonal classroom for the teacher in linking pupil inputs to school outputs. The environment of the classroom is intensely interpersonal, and good teaching is not simply a matter of using time wisely, of selecting the right reading series, or of adopting

<sup>18</sup> This contrasts with research at the secondary level, which typically finds that the influence of teachers as significant others falls far short of that exercised by friends and parents, and often concludes that they are not important at all (Alexander, Eckland, and Griffin 1975; Sewell, Haller, and Ohlendorf 1970; Williams 1972). The social organization of the elementary school into self-contained classrooms, though, also contrasts with the multiteacher schedules of secondary students.

<sup>19</sup> Hence, both administrative and developmental considerations identify the primary grades as an especially promising locale for teacher intervention, and this seems to be indicated as well in the growing literature on teacher effects at the elementary level (see, for example, Bossert 1979; Brown and Saks 1975; Pedersen, Faucher, and Eaton 1978; Summers and Wolfe 1977).



a particular classroom management technique, despite the tendency to cast school improvement policies in such terms.<sup>20</sup> Nor is it reducible to matters of professional development, for educational background and experience have proven in many studies to be of little importance in distinguishing effective from ineffective teachers.

All these perspectives neglect differences of teacher background and/or personality that determine to a considerable extent what actually happens in the classroom. Teachers implement the curriculum, regulate time usage, and structure classroom process. Whether they are sympathetic or hostile, faithful or lax, skillful or inept surely matters. Our results emphasize the social-psychological dynamics that underlie classroom process; pupil performance is driven down where teachers are distant and disaffected. The situation of high-status teachers working with disadvantaged youngsters is but a particular instance of this general proposition. But the conditions that give rise to such sentiment are themselves socially structured, and this transforms what otherwise would be simply a personal problem into a social one.

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<sup>20</sup> Barr and Dreeben (1983) come close to committing this error in their otherwise excellent study of instructional practice in the primary grades. See Karweit (1985) for a useful corrective to such mechanical thinking in the "time-on-task" literature.

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# NONVERBAL BEHAVIOR, DOMINANCE, AND THE BASIS OF STATUS IN TASK GROUPS\*

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*Linking nonverbal behavior to influence in task groups has been interpreted as evidence that behavioral dominance is the basis of status. Challenging this interpretation, this paper proposes that both the power processes that underlie status formation and the structural implications of dominance hierarchies indicate that expectations about task performance will be the usual basis of status in task groups. Furthermore, while some nonverbal behavior communicates dominance, it is not linked to influence. Influence results from nonverbal task cues that affect the performance expectations of an actor. An experiment tested this hypothesis by measuring the influence achieved by a female confederate in a three-person female group. As expected, the confederate was most influential when she displayed high-level task cues. When she displayed a high level of dominance cues, the confederate was not more influential than when she displayed submissive or low-task cues. The results suggest that status is a collective product of the entire network of group members, rather than an aggregate of pairwise competitions among members.*

Scholars currently debate the development of informal status hierarchies in task groups: are they based primarily on factors related to task performance or on behavioral dominance? This important question asks whether face-to-face status results from a collective, cooperative process even if this process, like conformity, has a coercive edge. Or is status simply the aggregate result of an individually oriented competitive process? Since task groups are the major decision-making entities in complex societies, it is important to understand the informal hierarchies of influence that substantially determine their decisions. Recent evidence linking nonverbal behavior to influence is sometimes interpreted as supporting the dominance view (Lee and Ofshe 1981; Lamb 1981; Rosa and Mazur 1979; Mazur et al. 1980; Mazur 1985). This paper contests that interpretation by testing arguments (Ridgeway 1984) that support the performance approach.

Most sociological theories view status hierarchies as developing from the *cooperative* interdependence among the group members who share a task (Bales 1950; Homans 1961; Berger

et al. 1974). Under such conditions, say these theories, it is in each member's self interest to defer to others, based on relative expectations for task performance, so that the group may generate higher rewards for all members through greater success at the task. Thus, deference is exchanged for anticipated superior task contributions. Members form expectations about the relative quality of each other's task contributions from one another's status characteristics (e.g., sex or race), reputation for competence relevant to the task, and actual behavior in the situation (Berger et al. 1974; Berger et al. 1977; Berger et al. 1986).

Theories focusing on dominance behavior challenge this view by arguing that status hierarchies in task and other face-to-face groups largely result from *competitive* interdependence induced by basic behavioral impulses for power or dominance over others. Such impulses encourage members to display assertive dominance behaviors towards others to elicit deference from them. Lee and Ofshe (1981) suggest that dominance behavior calls out deference directly in a learned, stimulus-response fashion, creating a direct status ranking between the interactants without regard to shared task needs.

Mazur's (1985; Rosa and Mazur 1979; Mazur et al. 1980) more complex view of status formation incorporates cooperative factors as well as dominance behavior. He argues that status differences are created by the display of "status signs," which include constant attributes of individuals, such as age or wealth, and controllable gestures called dominant and deferent acts. When one person displays high status signs toward another, the other may defer

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immediately for many different reasons (called cooperative status allocation) or engage the first person in a dominance contest that ends when one person succeeds in "outstressing" the other through stress-inducing dominance acts (called competitive allocation). Mazur does not address the relative impact of each mode of allocation on the formation of hierarchies. However, to the extent that pairwise dominance contests such as he describes do create the status hierarchies of task groups without regard to task concerns, his approach presents problems for the performance view.

To the degree that various theories emphasize dominance behavior as a basis of status differentiation in task groups, then, they argue that, although members may share an interest in the task, they have a stronger and more basic interest in maximizing their personal power over others. This interest in power rather than collective interest, will determine individual deference behavior in task as in other groups. This view of status differentiation can imply that performance theories rest on an excessively voluntaristic view of the formation of status orders that underestimates their implicitly coercive aspects.

#### POWER, PERFORMANCE, AND STATUS

I argue here that this implied argument is in error, at least in regard to task groups. To see why, let us consider a group of three or more people with a shared task goal and a recognition that those given high status will disproportionately affect task success, as is the case in human task groups (Mazur 1973).<sup>1</sup> These initial conditions create a positively connected exchange network among the members (Cook and Emerson 1978; Emerson 1981).<sup>2</sup> Such a network exists if people are connected in two or more exchange relations such that exchange in one relation is contingent on exchange in another. In the group in question, deference granted by one member ( $M_1$ ) to a second member ( $M_2$ ) results in task services performed by  $M_2$  that yield benefits (or losses) not only to  $M_1$  but to all other members ( $M_3-M_n$ ). Thus

the exchange of benefits between  $M_2$  and  $M_3$  is contingent on the exchange of deference between  $M_1$  and  $M_2$ , and vice versa, resulting in a positively connected network.

This positively connected network creates a power structure that makes performance expectations the usual basis of status in task groups. The contingencies of such a network make it in the interests of each member that all *other* members grant deference on the basis of expectations for task performance rather than on another criterion. Therefore, if other group members have higher expectations for  $M_2$ 's task performance than  $M_1$ 's, they are likely to pressure  $M_1$  to defer to  $M_2$ . The possibility of such a coalition forming creates a power structure that establishes and enforces inferred performance capacity as the appropriate basis for "voluntary" deference and status.<sup>3</sup>

As Emerson (1962, 1972) notes, group norms are properly viewed as the collective "voice" of a coalition that would stand against a member who violates the norm. Since the positively connected network creates the structural likelihood of coalitions forming in support of deference to high performance signs, these potential coalitions, in effect, create norms defining expectations for task performance as the legitimate basis of status. Legitimacy allows high-status members to act with group support to defend their position in the group. This may include directing dominance behavior (for example, shouts, commands, and aggressive stares) toward insubordinate lower-status members or "illegitimate" (i.e., low-performance sign) challengers. These legitimating norms also mean that the display of dominance behavior in an effort to seize status without regard to performance expectations will likely be treated as a violation to be resisted by other group members.<sup>4</sup>

<sup>1</sup> Dyads are excluded from the analysis because their power and exchange relations have specific limitations (Cook and Emerson 1978).

<sup>2</sup> Emerson (1962, 1981) distinguishes between networks and groups. However, he considers that small groups begin as networks and are transformed into groups, as he uses the term, when norms develop (Emerson 1962, 1972). Although I am discussing a group in the formative stage, before its normative structure has emerged, I assume that, after the group has developed, the exchange network remains as a potential structure that sets certain bounds on normative relations. This assumption differs somewhat from Emerson's view.

<sup>3</sup> It is important to emphasize that this potentially coercive power structure supporting deference to performance signs is a structural effect of the exchange network created in a task group. It does not depend on the members' personal preferences for their own status attainment. Even if dominance arguments are correct in asserting that people's fundamental impulse is for their own personal dominance, these same people in a task group should want *others'* status to be granted on the basis of expected task performance.

<sup>4</sup> This analysis suggests that the formation of status orders in groups of three or more is a *collective* process emerging out of the conjoint behavior of the entire network of members. Approaches focusing on dominance behavior, on the other hand, usually view a status hierarchy as an *aggregate* result of a series of dyadic dominance contests among the members (Strongman and Champness 1968; Jones 1983). Chase (1974, 1980), however, has shown that the correlations between individual differences in dominance-related attributes and

## DOMINANCE AND STATUS

Dominance behavior, defined in general agreement with most students of dominance (Strayer 1981; Jones 1983; Mazur 1985) as behavior directed toward the control of another through implied or actual threat, does occur in task groups.<sup>5</sup> If a transitive status hierarchy in a task group were to be based entirely on such behavior, what pattern of dominance attacks and reactions would have to occur? Let us assume a member attempts to seize control of the group through such behavior. To succeed, the member must establish a dominance hierarchy in the initial moments of interaction before the positively connected network among the members could create norms for performance-oriented status allocation. This will only be possible if the dominator can keep other members from forming a coalition in opposition to his or her efforts to take over the group by threat. Blocking such coalitions requires that the dominator engage in rapid series of successive dominance contests (shouts, stare-downs, etc.) with several members, "winning" by extracting deference in each case. Furthermore, deferring members must react to defeat, not by banding together, but by attacking each other or withdrawing from the fray (Chase 1980). In this situation, a pure, transitive dominance hierarchy would be formed.<sup>6</sup>

status hierarchy rank are not high enough among either people or animals for this dyadic-aggregate approach to account for the near linear or transitive nature of most human and animal status hierarchies. Instead, he argues that one must consider the pattern behavior among all group members to account for linear hierarchies.

<sup>5</sup> There is no accepted definition of human dominance behavior (Strayer 1980; Harper 1985). Strayer (1981) and Jones (1983) focus on dyadic conflict interactions that are agonistic or coercive because they involve physical aggression, the threat of aggression, or struggles for objects or physical position. Mazur (198) defines dominant acts as stress-eliciting behaviors, but notes that they have this effect because they are perceived as threatening. My definition employs the common thread in these definitions: the effort to induce compliance or otherwise control another through threat. The credibility of the threat implied by a dominance behavior varies with the characteristics of the dominator (e.g., size or status) and the context. Given the proscription against physical force in most task groups, the credibility of a threat will most likely lie with the social power the dominator has to coerce the challenged member. Thus, dominance behavior may most likely be used successfully by an already high-status person towards a low-status person. Among peers, a dominator's threat becomes more credible if implicitly backed (or not opposed) by the coalitional support of others, suggesting again the importance of bystanders for dominance contests.

<sup>6</sup> Besides presuming this complex sequence of behavior by both contestants and bystanders, a status system based on a dominance hierarchy has certain structural

While possible, such a sequence of behavior in task groups is rather rare. The evidence indicates that most status allocation in groups is cooperative rather than contested in that deference is given without visible contest or without explicitly threatening or agonistic behavior from the recipient of deference (Mazur 1973, 1985). Consequently, most actual dominance contests occur as a breakdown of a cooperative status allocation process (Mazur 1985). According to our analysis of positively connected networks, this means that dominance contests in task groups generally occur in the context of members operating according to the assumptions of a performance-oriented status process. In this context, a dominance contest is not an isolated struggle between two members. Since the winner will have a greater impact on the collective task effort, it involves the interest of all group members. As a result, rather than remain disinterested bystanders, other group members are likely to lend their coalitional support to the contestant they feel will benefit them the most.

Whether or not they actively intervene to determine the outcome of the dominance contest itself, bystander members are likely to determine whether to acknowledge this struggle between two members and allow it to become part of the group status hierarchy. Members acknowledge this struggle as part of the hierarchy by granting influence to or withholding it from the dominator. Consequently, the ultimate success of a dominance attempt in achieving status depends on winning coalitional backing from other group members, not primarily on extracting submissive signals from the challenged member. Given their self interests, bystander members are likely to support or resist a dominator, not on the basis of threat capacity, but on the basis of his or her expected performance potential.<sup>7</sup> There is evidence that

shortcomings. Since they are created independently of collective-task interests, they do not themselves organize task activities or generate additional resources for the group (for a fuller discussion, see Ridgeway 1984, 1986). Thus, they cannot explain why the status systems of task groups are nearly always the organizing agent of task activities. Ethologists have also become increasingly aware of the limitations of the dominance argument to explain the hierarchies of higher primates. Mitchell and Maple (1985) note mounting evidence of the importance of collective, cooperative behavior in such hierarchies and conclude that an individual animal's rank seems to be determined by "social skills" such as the ability to form alliances, as well as the ability to manipulate others.

<sup>7</sup> Unless the dominator is already higher status than they, bystanders are unlikely to feel personally threatened by the dominator since he or she is displaying dominance behavior that is not only *not* directed toward them, but which is usually nonagonistic. Mazur (1973, 1985) notes

bystander members are also influenced, to a lesser extent, by perceptions that a challenger is motivated by a cooperative desire to aid the group's task effort (group-oriented motivation), rather than by pure self-interest (Ridgeway 1978, 1982, 1984).

The question, then, is how dominance behavior affects the groups's assessment of the dominator's performance capacity and group or self-motivation. Mazur (1985) and Lee and Ofshe (1981) imply that differences in dominance behavior might, in some unspecified way, give rise to corresponding differences in apparent task capacity. This is a question we will address empirically. However, there are reasons to disagree with this suggestion. Dominance behavior provides little direct evidence by which to judge task competence because it claims status on the basis of threat rather than makes a case for the dominator's task contributions. It also usually appears to be self-motivated (Ridgeway 1978). By this analysis, the display of dominance behavior alone should not be very successful for achieving status and influence in a task group.

#### NONVERBAL BEHAVIOR, DOMINANCE, AND COMPETENCE

How can studies linking assertive nonverbal behavior to influence in task groups be explained if the foregoing argument is true? Some common assertive behaviors, such as silently staring another down, shouting, or commanding another are indeed dominance behaviors since they seem to be attempts to control through threat. However, these behaviors have not been empirically linked with status attainment. Actual correlates of status include duration of initial eye contact, speaking first or responding quickly in conversation, choosing the head of the table, verbal fluency, speech rate and a "confident" voice tone (Lee and Ofshe 1981; Rosa and Mazur 1979; Willard and Strodbeck 1972; Nemeth and Wachtler 1974; Street and Brady 1982). These behaviors may not be interpreted as efforts to control through threat since they do not suggest that the other should submit "or else." They may, however, affect assessments of the actor's performance capacity (Ridgeway 1984; Ridgeway et al. 1985). Berger et al. (1986) argue that such behaviors act as *task cues* that make claims or permit inference about how well the actor will do or is doing at the task. Evidence shows that people who present their ideas with a high level of these task cues (e.g., a

sustained gaze, a quick verbal response, rapid speech, and a confident voice tone) "sound better" and are often judged to be more competent (see Berger et al. 1986 for a review).

Task and dominance cues, then, may be distinct dimensions of nonverbal behavior in task groups. Those in the task dimension but not those in the dominance dimension affect performance expectations and, consequently, influence in the group. If the argument is correct, first, bystander members should grant greater influence to a member when that member displays high-task cues in a pairwise exchange with a third member than when the member displays low-task, dominance, or submissive cues. Second, such a member should not attain significantly more influence when displaying dominance cues than when displaying submissive or low-task cues. Third, such a member should be perceived as more competent at the task when displaying high-task cues than when displaying dominance cues, but more self-oriented when showing dominance cues.

#### VERIFYING THE ARGUMENT

##### *Design and Procedures*

To test these hypotheses, I designed an experiment to assess the impact of a target confederate displaying one of four cue patterns (high- or low-dominance cues or high- or low-task cues) on a third task-group member who is a bystander to a pairwise exchange between the target confederate and a second member, also a confederate. Using procedures adapted from Nemeth and Wachtler (1974) and Lee and Ofshe (1981), the target and second confederate, both student actresses, were videotaped while arguing a jury case about the size of a financial award (from \$0 to \$25,000) in an insurance settlement. While displaying a specified cue pattern, the target argued for a \$2,000 award, shown by previous research to be atypically low in comparison to that favored by most undergraduates (Nemeth and Wachtler 1974). The second confederate argued for \$15,000, generally an average award choice, while maintaining a neutral level of dominance and task cues. Videotapes were used to ensure control and consistency in the pattern of nonverbal behavior displayed by the confederates. There were four videotapes in all, differing only in the nonverbal cue pattern displayed by the target actress.

To avoid the potentially confounding effects of external status and gender differences, all subjects, were female undergraduates randomly assigned to one of four conditions according to the videotape they would see. They were told they were participating in a study of individual and group decision-making. On arriving, sub-

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that human dominant acts vary from highly stressful attacks and explicit threats to subtle, less stressful acts such as stares and violations of conversational rules. The latter are typical.

jects individually read a description of the jury case and made an initial award decision using a scale marked at \$0 and at \$2,000 intervals from \$1,000–\$25,000. They also indicated the highest and lowest award they would assent to for the sake of group agreement. To allow for an adequate range in the measure of the target's influence only subjects who chose an initial award greater than \$11,000 were included in the final design.

Subjects were then seated in individual carrels to watch a videotaped experimenter explain the group phase of the study. They were told they would work on the same jury case as a member of a three-person group. The group, however, would be a new type of decision-making group, called an "extended communication group" and made possible by modern electronics. Their two groupmates would be videotaped undergraduates from their university, who would discuss the jury case. Subjects were to listen to their groupmates' arguments carefully, and make a final award. Their final decision would be combined with those of their videotaped groupmates into a "group decision profile," which would be evaluated against those produced by legal experts to assess their group's performance as poor, average, or superior. They would be informed afterwards how well their group had done. Finally, subjects were told that, in a future phase of the study, they would meet their teammates face-to-face and work with them on another jury problem for which they, too, would be videotaped.

These instructions were designed to encourage subjects to feel part of their group despite the necessary use of videotapes to reliably control nonverbal presentations by the confederates. The instructions also encouraged the subjects to take the task seriously and consider their teammates' views in making their own decisions, both necessary conditions for the development of a performance-based status structure. To ensure that these conditions were met, and particularly that subjects genuinely believed they were part of a group, probe questions on the post-experimental questionnaire were explored in a post-experimental interview with each subject. Of the 90 subjects, only four doubted the reality of the situation or said they did not feel a part of the group, two failed to consider their teammates' views in making their decisions, and one did not take the task seriously. Data from these seven subjects were excluded. Final results were based on the 83 subjects who accepted the reality of the group and were oriented toward the task and the group.

After viewing the instructions, subjects in condition one ( $n = 20$ ) watched a videotaped group discussion in which the target confederate

displayed high-task cues. Those in condition two ( $n = 21$ ) saw the target use dominance cues, those in condition three ( $n = 21$ ) saw low-task cues, and those in condition four ( $n = 21$ ) saw submissive cues. At the conclusion of the taped discussion, subjects made a final award decision using the same \$0–\$25,000 scale, again indicated the highest and lowest award that they would now agree to, and completed a post-experimental questionnaire evaluating the target.

*Measures.* The primary measure of the major dependent variable, influence of the target, was the difference between the subject's initial and final award levels. Equivalent differences on highest and lowest acceptable award provided secondary influence measures. Since the subjects selected had initial awards greater than \$11,000 and the target argued for \$2,000, a reduced final award indicates target influence.

The target's apparent task capacity, dominance, and group- versus self-motivation were measured by a series of seven-point Likert and semantic differential items on the post-experimental questionnaire. Subjects rated the targets' skill at the group task, how effectively the target argued her case, how knowledgeable she was about the task, how convincing, competent, intelligent, and persuasive she appeared, and whether she had high- or low-quality ideas. These together comprised the task-capacity items. Group- versus self-motivation items asked the subjects to rate the target as reasonable, fair, and modest (versus egotistical), as well as group- versus self-oriented (defined as motivated by desire to help group decision making as opposed to motivated by self-interest). Dominance-related items assessed the target as tense or relaxed, intimidating or fearful, aggressive or unaggressive, and dominant or submissive. Socioemotional items rated the target as pleasant, likable, and negative (versus positive). Finally, subjects rated the target's degree of leadership ability and apparent high or low status.

*Experimental Videotapes.* Theoretical definitions of dominance and task behavior were used to develop four cue patterns for the experimental videotapes. Nonverbal and paraverbal dominance behaviors can be distinguished by their communication of threat. Previous research suggests that a loud voice with an angry, imperative tone, overt staring, particularly with lowered eyebrows, a straight, looming posture with high muscle tension, and pointing, intrusive gestures are likely to communicate threat and dominance (Mazur et al. 1980; Keating et al. 1981; Leffler et al. 1982; Lee 1979). Low dominance, or submissiveness, is indicated by opposite cues, such as a soft, tremulous voice with a pleading tone, averted eyes with

Table 1. Nonverbal Cue Pattern by Condition

Condition	1	2	3	4
Pattern	High-task	Dominant	Low-task	Submissive
<i>Cue</i>				
Voice volume	Medium	Loud	Soft	Soft
Voice tone	Factual, confident	Commanding	Uncertain	Tremulous, fearful
Speech rate	Rapid	Medium	Slow	Medium
Hesitations	Few	Medium	Many	Medium
Stumbles	Few	Medium	Many	Medium
Speed of response	Fast	Medium	Slow	Medium
Eye contact	High w/normal break-offs	Staring	Low	Averted w/sneaking glances
Eyebrows	Level, normal	Lowered, glaring	Level, normal	Knitted
Posture	Straight, relaxed	Tense, looming forward	Slumped	Slumped, cowering
Gestures	Few confident	Intrusive	Few uncertain	Nervous

occasional sneaking glances at the other, knitted eyebrows, nervous gestures such as hand wringing or grooming behavior, and a tense, slumped, cowering posture (Mazur 1985).

As these lists suggest, the element of threat gives both dominance and submissive cues a tense, highly emotional quality. This is the stress-related aspect that Mazur (1985) refers to. Task cues, on the other hand, are more neutral in emotion, projecting confidence or uncertainty rather than threat or fear. A factual voice tone, fluent, rapid speech with few hesitations or stumbles, quick verbal reactions, a steady direct gaze with normal break-offs, and a straight but relaxed posture communicate high performance capacity. Low-task cues are a hesitant, uncertain voice tone, slow speech with frequent stumbles and hesitations, slow verbal reactions, a low level of direct eye contact, and a slumped but not too tense body posture. Table 1 summarizes these four cue patterns.

The four videotaped stimuli differed only in the cue pattern displayed by the target confederate. Except for opening and closing shots showing the two undergraduate actress-confederates exchanging views across a small table, the tapes consisted of alternating close-ups of the second confederate and the target (always played by the same actress) giving equal-length arguments in favor of their chosen award. To ensure that the neutral second confederate remained consistent over all four tapes, a single videotaped performance by the second actress-confederate was edited into all but the opening and closing shots of each tape. The voice volume of the target relative to the second confederate and the target's speed of response were mechanically edited to vary as specified in Table 1.

*Validating the Videotapes.* Since the actress-confederates were females, as were the subjects, each videotape was pretested on separate groups of 13 to 26 female undergraduates (for a total of 71 pretest subjects). After viewing a tape, they rated the target's nonverbal cues and apparent

dominance and competence on seven-point scales (see Table 2). Descriptive *t*-tests on mean ratings of each target confederate's nonverbal cues were used to evaluate the accuracy of the actress's portrayal of the four cue patterns.<sup>8</sup> Ratings of the target's dominance and competence were used to assess the conceptual validity of the pattern of cues assigned to dominance and task categories.

Results of these tests indicated that the dominant target, compared to both the high-task and submissive targets, had a significantly ( $p < .03$  for all comparisons) louder, firmer, angrier voice tone, a sterner facial expression, and more intrusive gestures (see Table 2). Compared to the high-task target, the dominant target was also seen as having a similarly straight but significantly ( $p < .03$ ) less relaxed, more forward-leaning posture. The dominant and high-task targets were not rated differently in the amount of direct eye contact they maintained while speaking.<sup>9</sup> However, the dominant target was perceived as maintaining substantially ( $p < .01$  for all comparisons) more direct eye contact, adopting a more upright, forward-leaning posture, and engaging in fewer nervous gestures than the submissive target. As

<sup>8</sup> These significance tests are for descriptive purposes only because the four videotapes represent the entire relevant population, rather than a sample on the basis of which inference is made to a larger population.

<sup>9</sup> Pretest subjects were inadvertently not asked to rate eyebrow level, a key to distinguishing dominant from high-task eye contact (Keating et al. 1981). Consequently, experimental subjects were asked to rate (on a seven-point scale) the extent to which the target stared at the other with lowered brows. As expected, the dominant target was rated significantly higher ( $p < .01$  for all comparisons) than all others (5.81 versus 2.63 for high-task; 2.90 for low-task; and 1.95 for submissive). The submissive target was rated lower than all others (see Table 3). Thus, while the high-task and dominant targets were rated similarly in total eye contact, they differed in the nature of that contact, with the dominant target staring with lowered brows.



Table 2. Mean Pretest Ratings of Videotapes

Condition <i>n</i>	1 13	2 26	3 19	4 13
Target	High-task	Dominant	Low-task	Submissive
<i>Cues</i>				
Voice volume: loud	4.54	6.31	3.33	3.31
Voice tone:				
firm	5.46	6.58	1.33	1.92
whining or pleading	1.92	2.81	4.26	5.62
angry	3.00	6.73	2.22	1.85
Speech rate: fast	5.15	5.65	1.94	2.31
Hesitations	1.77	2.84	6.78	6.08
Verbal stumbles	2.62	3.42	6.78	5.92
Speed of response	6.31	6.08	2.21	3.00
Eye contact: direct while speaking <sup>a</sup>	6.23	5.58	2.11	2.00
Facial expression: stern	4.00	6.73	1.84	1.15
Posture:				
relaxed	3.77	1.42	1.68	1.00
leaning forward	5.23	6.31	4.63	4.38
upright vs. slumped	5.23	6.00	2.37	2.31
Gestures:				
nervous	2.38	3.35	6.37	7.00
intrusive (e.g. pointing)	3.77	5.31	3.21	1.77
<i>Evaluations</i>				
Intimidating	3.92	5.46	2.63	2.54
Demands agreement	5.15	6.42	2.52	2.00
Dismissive of other	4.62	5.00	3.89	1.92
Threatening	4.38	5.58	1.56	1.15
Intelligent	4.08	3.46	2.95	3.00
Estimated GPA	4.00	3.42	3.00	2.92
Group-oriented	5.15	3.08	4.37	4.54

<sup>a</sup> See note 9 for further explanation of eye contact differences.

expected, the dominant tape showed more dominance cues than the high-task tape, in which a medium level was expected, and a much higher level than the submissive tape, in which low dominance cues were expected.

Relative levels of these behavioral cues did indeed give the impression of corresponding degrees of dominance as indicated by the subjects' more general evaluation of the different targets' degree of dominance (see Table 2). The dominant target was rated ( $p < .04$  for all comparisons) as expressing her views in a more intimidating fashion, being more demanding of agreement from others, and being more threatening in her self-presentation than the high-task and submissive targets.

As expected, the high-task target was rated highest overall in task cues, and was seen as dramatically higher in these cues than the low-task target (see Table 2). However, the differences between the high-task and dominance targets on task cues was not as great as on dominance cues. The high-task target was rated as having fewer hesitations while speaking ( $p < .02$  and  $p < .001$  respectively) and stumbling less over her words ( $p < .10$  and  $p < .001$ ) than the dominant and low-task targets. While the high-task target was also rated as having a faster speech rate and responding more quickly than

the low-task target ( $p < .001$  for each), she did not differ on these variables from the dominant target. As expected (see Table 1), the high-task target also had a louder voice, a more upright posture, and maintained more direct eye contact ( $p < .001$  for each) than the low-task target.

It appears that the actress portrayed much higher levels of task cues on the high- than on the low-task tape, but, on the dominant tape, was only slightly lower in task cues than on the high-task tape. Nevertheless, the high-task target's slight advantage over the dominant target in rated task cues appeared sufficient to give an impression of greater general competence. Levels of task cues displayed corresponded to levels of perceived competence, supporting our conceptual analysis of their effects. Although each was portrayed by the same actress giving the same arguments, the high-task target was rated as more intelligent ( $p < .01$ ) and estimated to have a higher grade-point average ( $p < .04$ ) than both the dominant and low-task targets. Thus, cues classified as task and dominance did appear to create differing impressions of the target.

As we have seen, the low-task and submissive targets differed substantially and appropriately from the high-task and dominant targets respectively, providing the contrast necessary to test

Table 3. Mean Awards by Target Cue Pattern

Condition Cue Pattern	1 High-task	2 Dominant	3 Low-task	4 Submissive
<i>Dollar Award</i>				
Initial	\$18,600.00	\$19,000.00	\$17,761.91	\$18,238.10
Final	11,850.00	14,047.62	13,714.29	14,428.57
Adjusted final*	11,729.42	13,688.76	14,092.97	14,523.58
<i>Highest Acceptable Award</i>				
Initial	\$22,780.00	\$22,666.67	\$21,666.67	\$22,571.43
Final	15,350.00	19,428.57	17,523.81	17,952.38
Adjusted final*	15,068.96	19,246.58	18,084.70	17,841.14
<i>Lowest Acceptable Award</i>				
Initial	\$11,150.00	\$11,857.14	\$11,761.91	\$11,476.19
Final	7,700.00	8,857.14	8,761.90	8,380.95
Adjusted final*	7,950.99	8,681.76	8,643.94	8,435.26

\* Adjusted for initial award in an analysis of covariance.

the hypotheses. Although the submissive and low-task targets did not differ greatly from one another, the differences that did occur indicated higher submissive and lower dominance cues in the submissive target, as expected.

These results indicate that the four cue patterns were portrayed with sufficient accuracy to allow a clear test of the experimental hypotheses. That the dominant target was only slightly lower in task cues than the high-task target and the submissive target was barely higher on these cues than the low-task target do not pose difficulties. If influence is indeed due to the task rather than dominance dimension, and if dominance behavior does not enhance apparent task capacity but has negative motivational implications, then the dominant target should still have lower influence than the high-task target as predicted. In any case, the dominant target's relatively high level of task cues should bias the results against, rather than for, the hypotheses. Similarly, the relative similarity of the low-task and submissive targets is not problematic because no differential effects were predicted between these cue patterns.

### Results

If the argument is correct, the high-task target should have greater influence than other targets, and the dominant target should not differ in influence from the submissive and low-task targets. Because initial award levels differed between groups, and because the assumption of equal slopes was met, analyses of covariance on final award levels, with initial awards as the covariate, were used to assess these predictions (see Table 3). Following significant initial *F*-tests, a priori *t*-tests on the adjusted final award means confirmed that subjects who viewed the high-task target lowered their final awards (indicating influence) significantly more

than did subjects in other conditions (high-task versus dominant:  $t = 1.69$ ,  $d.f. = 78$ ,  $p = .05$ ; versus low-task:  $t = 2.00$ ,  $p < .025$ ; versus submissive:  $t = 2.37$ ,  $p = .01$ ).<sup>10</sup> Importantly, the high-task target had greater influence than the dominant target. Although the dominant target had unexpectedly high task-cue levels, it appeared that her strong dominance behavior may indeed have produced some resistance because also as predicted, she did not achieve significantly greater influence than the submissive ( $t = 0.72$ ,  $d.f. = 78$ ,  $p = .48$ ) or the low-task targets ( $t = 0.35$ ,  $p = .72$ ).

Means for the secondary influence measures, highest and lowest acceptable awards, were also ordered as predicted. On the highest-acceptable-award question, subjects in the high-task condition lowered their awards (indicating greater target influence) substantially more than those in any other condition (high-task versus dominant:  $t = 2.94$ ,  $d.f. = 78$ ,  $p < .003$ ; versus low-task:  $t = 2.10$ ,  $p < .02$ ; versus submissive:  $t = 1.95$ ,  $p < .03$ ). Furthermore, subjects in the dominant condition, as predicted, did not differ from those in the submissive or low-task conditions in their final choice for highest acceptable award (dominant versus submissive:  $t = 1.00$ ,  $d.f. = 78$ ,  $p = .32$ ; versus low-task:  $t = .82$ ,  $p = .41$ ). Means for lowest acceptable

<sup>10</sup> A priori *t*-tests following significant initial *F*-tests on the primary and secondary influence measures are required for a precise test of the hypotheses because they specify a set of pairwise comparisons between targets. However, because some of the comparisons required by the theoretically derived hypotheses are nonorthogonal, there is some danger of accumulating error rate in this technique. Because the comparisons are theoretically predicted, the risk of capitalizing on chance by this means is not as serious as it would be if they were post hoc. Nevertheless, keep in mind that, with multiple significance tests, one in twenty will be significant by chance alone.

award followed the same pattern, but the differences were not significant. Interestingly, on these two measures the dominant target actually had the lowest apparent influence (indicated by higher awards), while the high-task target had the highest.

Subjects' ratings of the targets on the questionnaire items (see Table 4) indicate that the failure of the dominant target to achieve significant influence cannot be attributed to an insufficiently dominant appearance. In fact, the dominant target was rated 6.57 on the seven-point dominant-submissive item (see Table 4). The t-tests showed that the dominant target was perceived as significantly more dominant ( $p < .001$  for all comparisons), more intimidating ( $p < .04$  for all comparisons), and more aggressive ( $p < .01$  for all comparisons) than all other targets. The tense-or-relaxed question was a validating item derived from our definition of dominance cues as centered on threat; a person expressing dominance through threat was tenser and more emotional than one using task cues. In fact, the high-task target was rated as relatively relaxed, while the dominant target was fairly tense ( $t = 6.25$ ,  $d.f. = 39$ ,  $p < .001$ ). Similarly, the submissive target was tenser than the low-task target ( $t = 1.95$ ,  $p < .05$ ). That the experimental subjects rated the tapes in this way

on all dominance items, just as the pretest subjects had, suggests that the videotapes depicted a valid display of degrees of dominance behavior, and that the behaviors labelled dominance cues do, in fact, communicate a different message from that given by task cues.

I hypothesized that the high-task target would be more influential because expected task performance is the primary basis of status in task groups and because task cues affect apparent task capacity, while dominance cues do not. Subjects' perceptions of the targets' task capacities support this argument (see Table 4). The high-task target was seen as having more skill at the group task, arguing more effectively, being more knowledgeable, convincing, competent, intelligent and persuasive, and having higher quality ideas than the dominant or any other target. The t-tests on all but two (persuasive and knowledgeable) of the eight comparisons showed ratings of the high-task target to be significantly ( $p < .05$ ) higher than those of the dominant target. On the critical item of task skill, differences were especially strong ( $t = 2.96$ ,  $d.f. = 39$ ,  $p = .005$ ). If dominance cues had contributed positively to apparent task capacity, there should have been few differences between the dominant and high-task target on these items, especially given pretest evidence

Table 4. Mean Evaluations of Target by Cue Pattern

Condition Cue Pattern	1 High-task	2 Dominant	3 Low-task	4 Submissive
<i>Task Capacity</i>				
Task skill	4.50	3.05	1.95	2.05
Argues effectively	4.55	3.62	2.14	2.24
Knowledgeable	4.80	4.33	3.00	3.86
Convincing	4.70	3.43	2.10	2.14
Competent	5.70	4.00	2.40	2.81
High quality ideas	5.16	3.38	3.14	2.90
Intelligent	5.70	4.81	3.24	3.81
Persuasive	4.55	4.19	2.10	2.00
<i>Dominance</i>				
Submissive(1)-dominant(7)	5.35	6.57	2.38	1.76
Aggressive	5.20	6.62	2.00	1.33
Fearful(1)-intimidating(7)	4.95	5.67	2.29	1.76
Relaxed(1)-tense(7)	3.30	5.86	6.29	6.67
<i>Group Orientation</i>				
Self(1)-group(7)Oriented	3.85	2.00	2.81	2.76
Reasonable	3.65	2.24	3.57	3.38
Fair	3.90	3.00	4.05	3.90
Modest(1)-egotistical(7)	4.00	5.95	3.38	2.76
<i>Socioemotional</i>				
Pleasant	5.45	1.12	3.43	3.95
Likeable	5.35	1.86	3.19	3.57
Negative(1)-positive(7)	4.60	1.90	3.14	3.10
<i>Status</i>				
Low(1)-high(7)status	4.75	3.81	1.90	1.29
Leadership Ability	4.90	3.67	2.60	2.09

that the dominant target's task cues, while lower than the high-task target, were still rather high. The consistency of the high-task target's advantage on the task items indicates that dominance cues had either no or a slight negative effect on apparent task capacity. In fact, the relative ratings of all four targets on task capacity items corresponded with their relative level of task cues (i.e., the high-task target was highest, the dominant target somewhat lower, and the low-task and submissive targets were substantially and similarly lower). This ranking supports the validity of our distinction between task and dominance cues.

The dominant target was expected to have lower influence, not only because of lower apparent task capacity, but also because of resistance generated by the apparent self- rather than group-orientation. Table 4 shows the dominant target was seen as significantly more self-oriented ( $t=3.49$ ,  $d.f.=39$ ,  $p<.001$ ), less reasonable ( $t=2.74$ ,  $p<.01$ ), and more egotistical ( $t=4.18$ ,  $p<.001$ ) than the high-task target. The means of these items show the dominant target was distinctive in receiving consistent and fairly strong ratings of self-interest, while other targets were rated nearer to the scales' midpoints.

In addition to evaluating the target on behavioral dimensions derived from the theoretical argument, subjects also rated the target on socioemotional and status items. Given the self-interested appearance of the dominant target, it is not surprising that she was the most disliked of the targets (see Table 4). The extremely negative mean for the dominant target hints at the resistance I believe dominance behavior engenders in a task group of peers. The high-task target, on the other hand, gave the most positive impression, with the other two targets in between. All socioemotional differences between the high-task and dominant target were highly significant (for pleasant:  $t=9.95$ ,  $p<.0001$ ; for likable:  $t=7.85$ ,  $p<.001$ ; for positive:  $t=5.39$ ,  $p<.001$ ).

The two status-related items confirmed the picture of the high-task and dominant target indicated by other items. The high-task target was seen as having higher status (versus dominant  $t=2.94$ ,  $p<.01$ ; versus low-task:  $t=5.86$ ,  $p<.001$ ; versus submissive:  $t=7.52$ ,  $p<.001$ ) and possessing greater leadership ability (versus dominant:  $t=2.49$ ,  $p<.02$ ; versus low-task:  $t=5.94$ ,  $p<.001$ ; versus submissive:  $t=8.52$ ,  $p<.001$ ) than all other targets. The dominant target was significantly higher ( $p<.001$ ) on these items than the low-task and submissive-targets, but they differed from each other ( $p<.05$ ) only in leadership ability. It is interesting to note that Relative perceptions of status and leadership generally correspond to the

targets' relative level of task cues (high-task > dominant > low-task = submissive), rather than their level of dominance cues (dominant > high-task > low-task > submissive).

## CONCLUSIONS

The results of the experiment clearly supported the hypotheses. The distinction made between task and dominance cues in nonverbal behavior was clearly validated by subjects' perceptions of the relative dominance and task capacity of the four targets. The significance of this distinction was further enforced by the differential influence of the high-task target. As predicted, the high-task target was more influential than all other targets, while the dominant target was no more influential than the submissive or low-task targets. Furthermore, the high-task target was seen as more competent at the group task than the dominant or other targets, while the dominant target was seen as more self-oriented.

These results significantly support several aspects of the larger argument. They suggest first that the association between nonverbal behavior and influence in task groups is indeed due to task cues, rather than true dominance behavior, and that task cues affect influence by affecting perceptions of an actor's task competence. Second, they indicate that pure dominance behavior may not be an effective means for winning the deference (indicated by the granting of influence) from bystander members necessary to achieve and maintain higher status. Third, they suggest that dominance does not gain deference because it does not enhance performance expectations for the actor and seems self-interested, which, in a group of peers, arouses resistance. The support for these aspects of the argument, in turn, supports our fundamental assumption: status in task groups is based primarily on expectations about task performance, rather than on behavioral dominance.

This experiment tested the performance-oriented argument and was not designed to test directly the tenets of dominance theory; we cannot claim to have refuted those tenets. However, several results argue against a dominance interpretation. For instance, some dominance theorists might reject the task-dominance cue distinction, arguing that all assertive nonverbal cues are behavioral dominance. From such a viewpoint two predictions for this study could be made. (1) If it were assumed that the study randomly divided actual dominance cues into two undifferentiated subsets, then no differences in influence should exist between the high-task and dominant targets. (2) If it were noted that the dominant, compared to the high-task target, was distinctly higher in those

cues put in the dominance subset, almost as high on those in the task subset, and actually perceived as distinctly more dominant, then the dominant target should have greater influence than the high-task target. Both these predictions are contradicted by the results. Perhaps we need to reconsider the role of dominance behavior in task groups. Rather than a means of status attainment, it may be a prerogative of established status positions and may be used largely as a means of social control.

While the evidence of this experiment clearly favors a performance-oriented approach to status in task groups, some cautions should be noted. Since both the argument and data pertain to task groups, they say nothing about the role of dominance behavior in other groups. Second, the study examined influence, the principle, but not the only, component of deference. It is possible that dominance might still affect other components in other ways. Finally, the evidence here is from groups composed of women. To broaden support for the performance argument, the study should be replicated with men. Studies have found few differences in the relationship between dominance behavior and leadership in male and female groups (Jones 1983; Rosa and Mazur 1979; Lamb 1981; Mazur et al. 1980). This suggests that the pattern of results from male groups is likely to be similar to those here. If they are not, perhaps due to the relevance of dominance behavior to gender stereotypes, our argument here must be revised to apply only to females.

In supporting a performance approach, this study suggests that status hierarchies in task groups result from a collective process involving coalitions and substantial (but not complete) cooperation, rather than a purely individualistic "war of all against all" for dominance and power. What we observe in the status processes of executive committees, review boards, and other such task groups supports this conclusion. In these groups, influence is rarely seized or won through direct, individual competition or intimidation. Instead, it is usually established and maintained through cooperation with a coalition that, in turn, supports the member's influence attempts to the extent that they further the coalition's collective task interests.

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## RESEARCH NOTES

### PATERNAL PARTICIPATION AND CHILDREN'S WELL-BEING AFTER MARITAL DISSOLUTION\*

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*Using a nationally representative sample of children aged 11–16 who had experienced their parents' marital dissolution, we examine the influence of paternal involvement on the child's well-being. For measures of academic difficulty, problem behavior, and psychological distress, there is little evidence that paternal involvement had either harmful or beneficial effects. Paternal economic support reduced somewhat the likelihood of problem behavior. Frequency of visitation and closeness of relationship to father showed no consistent influence on the available measures of child well-being.*

The prevalence of divorce over the past decade implies that between a third and a half of all children in the U.S. will experience their parents' marital dissolution (Bane 1979; and Bumpass and Rindfuss 1979; Bumpass 1984; Furstenberg, Nord, Peterson, and Zill 1983). With this increasing incidence, concern has grown over the potentially deleterious consequences of marital dissolution for children. Prior to the 1970s, most researchers viewed divorce as a traumatic event that disrupted normal family functioning and was, therefore, likely to have negative behavioral, cognitive, and emotional consequences (see review by Herzog and Sudia 1973).

Led partly by empirical findings that did not show uniformly poorer outcomes for children experiencing divorce (Longfellow 1979; Thompson 1983), and partly by a growing recognition in developmental studies that most events do not produce a uniform response (Bronfenbrenner 1979), interest has recently shifted to the context and characteristics that condition children's responses (Furstenberg and Seltzer 1986). This paper focuses on one such aspect of marital dissolution, whether children are better off when

noncustodial fathers maintain an active role in their lives.

#### PATERNAL PARTICIPATION AND CHILD WELL-BEING

Few fathers retain custody of their children and most noncustodial fathers greatly decrease their involvement in childrearing. Many early investigations of the impact of divorce on children attributed developmental disabilities to this so-called syndrome of father absence. Recent research has expanded this theme, tracing the devastating economic and social consequences of divorce for female household heads and their children (Bane 1986; Bane and Ellwood 1983; Garfinkel and McLanahan 1985; Fuchs 1986). We have learned, for example, that mother-headed families are frequently below or near the poverty line (U.S. Bureau of the Census 1986), due, in part, to the unwillingness of nonresidential fathers to provide child support. Several U.S. surveys have shown that only a third of all single mothers receive regular child support; the contribution in most of these families is quite small (Weitzman 1985). Low levels of child support are typically accompanied by low levels of contact. Recent investigations have revealed that most fathers living apart from their children see them infrequently or not at all (Furstenberg et al. 1983; Furstenberg and Nord 1985).

Many researchers and policy makers believe that paternal absence and infrequent, irregular contact impede children's adaptation to divorce. Children deprived of paternal contact may grow up without a secure male model, may receive less parental support and supervision, and may

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be raised by a single mother who is under great stress. It is plausible, therefore, that children who have little or no contact with their noncustodial fathers will not fare as well as those who maintain an ongoing relationship (Weiss 1975, p. 217). Of course, the remarriage of the mother can greatly complicate this scenario.

The revolution in custody procedures that swept across the U.S. during the past decade was justified in part by the belief that children do better when both parents are actively involved in childrearing. The movement from maternal to joint custody was an attempt to expand the rights and responsibilities of fathers. Nevertheless, few studies have tested the hypothesis that frequent paternal contact aids children's adaptation to divorce.

In their study of approximately 60 families' adaptations to divorce, Wallerstein and Kelly (1980, p. 218) concluded that infrequent, irregular contact with nonresidential fathers usually led to feelings of rejection and lowered self-esteem. Nevertheless, they also discovered that, with age, the paternal relationship had a declining significance for the child's psychological well-being. Hetherington, Cox, and Cox (1978, 1979), in a study of 48 intact and 48 disrupted families, showed that children who maintained contact with their noncustodial fathers appeared to adopt more conventional sex-role patterns. Hess and Camara's (1979) study of 16 divorced and 16 intact families suggested that the child's relation with the resident mother and nonresidential father were equally important.

Although these studies are suggestive, their small and unrepresentative samples cast doubt on their conclusiveness. There are also reasons to suspect that the link between paternal contact and child well-being may be weaker than is generally believed. For example, regular contact between the child and nonresidential father may, in many cases, increase conflict between the ex-spouses, which could adversely affect the child. More generally, if the association between the quantity and *quality* of relations is weak, there may be little association between quantity and children's adjustment. Furthermore the father's participation may be influenced by the response of the child to divorce. For those children who adapt well, ties with their fathers may become closer over time. On the other hand, fathers whose children experience academic, behavioral, or psychological problems may withdraw from them. Finally, some fathers might become involved *because* their children are having adjustment problems. If such a variety of different causal sequences exists, the association between paternal contact and child well-being might in fact be very slight.

This paper examines whether children generally fare better when their noncustodial father

maintains an active presence in their lives. Further, we explore different dimensions of paternal involvement to see which is most critical for children. We also examine other factors that might weaken or strengthen the father's impact on the child.

## DATA AND METHODS

The data are drawn from the National Survey of Children (NSC), a panel study of a nationally representative sample of children interviewed in 1976 and 1981. Our analyses focus almost exclusively on the 1981 interviews, when the children were aged 11–16, because the information on paternal involvement is much richer. The interviews elicited a wide range of information from three sources—the children, a parent (almost always the mother), and a teacher—to assess the social and psychological functioning of the child. For further details on sample selection and data collection, see Furstenberg et al. (1983). Although the total sample included 1,423 children, we focus only on the 227 children who had experienced marital dissolution by the time of the 1981 survey, who were living with their mothers, and whose biological fathers were still alive.

Unlike many prior studies that focus on a single outcome or a single realm of psychosocial development, the NSC contains numerous items examining many areas of the child's functioning. This paper builds on two earlier analyses, which distilled a limited set of reliable indices that assess several dimensions of children's well-being (Furstenberg and Allison 1985; Furstenberg and Seltzer 1986). The names of the 10 measures of well-being can be found in Tables 1 and 2. Actually, these measures reflect "ill-being" since higher scores represent less desirable states or behaviors.

Except for the teacher's report in 1981, all reports of "academic difficulty" are based on single items measured on a five-point ordinal scale (treated as interval in the regression analyses). The remaining indicators are all multiple-item scales; the individual items and the estimated reliabilities are given in the Appendix.

### *Frequency of Paternal Contact*

Most fathers in our study did not see their children very often. As reported by the mothers, 23 percent of the fathers had no contact with the children during the previous five years. Another 20 percent did not see their children at all in the preceding year, 21 percent spent 1 to 12 days with their children in that year, 11 percent spent between 13 and 24 days, and the remaining 26 percent spent at least 24 days with their children. The children were also asked how often they saw their fathers; their answers were strongly associated with the mothers' reports, though the mean was somewhat higher. We



used the mother's report in our analysis, but the conclusions are not altered if one substitutes the child's report or a variable that combines the mother's and child's report. Further, results are very similar for other measures of contact with father, such as "How long since you last saw father?" or "How often do you talk to father on the telephone?"

The first column of Table 1 gives standardized coefficients from OLS regressions of each of the measures of well-being on paternal contact, coded as follows: 0 = none in past five year; 1 = none in past year; 2 = 1-12 days; 3 = 13-24 days; 4 = 25 or more days. The regressions also included controls for several background variables described at the foot of the table; coefficients for these variables are not reported.

Since the four categories of contact do not really constitute an interval scale, we also did analyses of covariance in which contact was treated as a set of five categories, again controlling for the same background variables. The *p*-values for the tests of the hypothesis that contact has no effect are given in the second column of Table 1.

It is apparent that these results provide little, if any, support for the hypothesis that paternal contact is beneficial to the child. None of the standardized coefficients is statistically significant at the .05 level. And although the hypothesis of beneficial paternal contact implies that the coefficients should all be negative, half are positive. For the analyses of covariance, paternal contact had a marginally significant effect on the mother's report of delinquency. But an examination of the coefficients (not shown) revealed an implausible pattern: children who had not seen their father in five years did

significantly better than those who spent between 0 and 13 days with their father in the previous year.

In separate analyses not shown here, we attempted to determine whether the effects of contact varied with the child's sex or with the current marital status of the mother. On the basis of previous research, we suspected that boys and girls might react differently to more active participation by the nonresidential father (Lamb 1977, p. 164-68). There were few significant interactions, however—no more than would be expected by chance. And those few that appeared did not occur consistently across similar measures, nor were they consistently in the predicted direction. Such a pattern of interactions could easily result from sampling variability.

The interaction of the mother's marital status with paternal contact also failed to clarify the picture. Some research has suggested that the mother's remarriage might complicate paternal participation, offsetting its beneficial effects for the child (Ganong and Coleman 1984; Pasley and Ihinger-Tallman 1987). We found no evidence to support this hypothesis.

In sum, we find that paternal contact is unrelated to a variety of well-being measures in the 1981 data. Similar analyses for measures of well-being constructed from the 1976 interviews were equally unresponsive of any effects of paternal contact. Apparently, children in maritally disrupted families were not doing better if they saw their fathers more regularly than if they saw them occasionally or not at all.

#### *Closeness to Father*

A possible explanation for these negative findings is that the quantity of paternal partici-

Table 1. Effects of Parental Contact and Closeness on Child's Well-Being

	Father's Contact		Father's Closeness		Mother's Closeness
	Interval $\beta$	ANCOVA <i>p</i>	Interval $\beta$	ANCOVA <i>p</i>	Interval $\beta$
<i>Mother's Report</i>					
Delinquency	-.03	.05*	.03	.00**	-.20**
Problem behavior	-.07	.86	.01	.70	-.18**
Distress	.09	.15	.06	.50	-.14*
Academic difficulty	.01	.91	-.02	.36	-.08
<i>Teacher's Report</i>					
Problem behavior	-.12	.54	-.09	.25	.05
Academic difficulty	-.06	.29	.04	.90	.11
<i>Child's Report</i>					
Delinquency	.04	.27	.03	.29	-.20**
Dissatisfaction	.11	.54	.06	.06	-.16**
Distress	.13	.24	.01	.23	-.22**
Academic difficulty	-.02	.98	.02	.60	-.02

Note: In addition to parental contact or closeness, the regressions included variables describing the child's age, race, sex, birth order, and region of residence, as well as the mother's education, religious preference, age at birth of the child, age at birth of first child, current marital status, foreign vs. U.S. birth, and timing marital dissolution.

\*  $p \leq .05$ .

\*\*  $p \leq .01$ .

pation may be only loosely indicative of the quality of relations between fathers and children. Children may closely identify with their fathers even though they see them infrequently or, alternatively, those who have frequent contact may experience greater conflict with their father or be subjected to rivalry between parents.

Those children who had seen their father in the last five years were asked how close they were to him. The distribution of responses was: no contact in five years = 23 percent; not very close = 10 percent; fairly close = 10 percent; quite close = 33 percent; extremely close = 22 percent. Given the generally low levels of contact, these reports are somewhat surprising. In fact, only a moderate correlation exists between the amount of time children spend with their noncustodial fathers and how close they feel to them. This moderate correlation is important in itself because it indicates the possibility that children can preserve identity with a parent whom they see infrequently.

The third column of Table 1 shows standardized coefficients for regression of each of the measures of well-being on paternal closeness, again controlling for the set of background variables. The five categories of closeness are assigned ordinal scores, which are treated as an interval variable. The fourth column gives *p*-values from analyses of covariance in which the categories of closeness are treated as an unordered polytomy. The results are quite similar to those for paternal contact: none of the standardized coefficients is statistically significant and only two of the ten are in the expected direction. Again, the analysis of covariance shows a significant effect of closeness on the mother's report of delinquency, but the pattern of coefficients (not shown) is completely inconsistent with the hypothesis.

In further analyses not presented here, we examined more refined hypotheses that might explain the absence of an association between closeness to the noncustodial father and children's adjustment. Specifically, we examined interactions to determine whether the effects of closeness varied with child's sex, closeness to mother, presence of a stepfather, and recency of separation. We found no evidence for any of these interactions. In short, we have been unable to specify a set of conditions in which the quality of child's relationship to his or her outside father seems to matter.

Perhaps this largely negative set of results merely indicates that our measures of well-being are unreliable or invalid. We are inclined to dismiss this possibility because many of the control variables are related to these outcome measures in predictable ways. For example, delinquency and problem behavior is more

common for boys and for those from low socio-economic statuses. Moreover when the children of divorced and separated parents are compared with those from intact families, the latter show consistently better outcomes on many of these measures (Furstenberg and Allison 1985). Finally, the last column of Table 1, which gives standardized coefficients for the child's report of closeness to *mother*, reveals a clear pattern of strong effects. Maternal relationships apparently *are* important for children's well-being.

#### *Economic Support from Fathers*

The weak effects of paternal contact and closeness suggest that the emotional significance of paternal participation may be overstated in much of the current policy deliberations about family relations after divorce. Could it be, as some have suggested (McLanahan 1985), that fathers' main influence is through their economic contributions?

In the NSC survey, mothers were asked for dollar amounts received from fathers during the previous year for both schooling and for all other purposes. Approximately 60 percent received no money whatsoever. Another 9 percent received less than \$1,200, 16 percent received between \$1,200 and \$2,400, and 16 percent received more than \$2,400.

Table 2 shows estimates of the effects of paternal contributions on children's well-being, controlling for other variables. As in Table 1, these were first estimated by assigning ordinal scores to the four levels of support, and then treating these as an interval scale. The standardized coefficients for these regressions are given in the first column. Significant negative effects appear for both the mother's report of problem behavior and the teacher's report of problem behavior. These coefficients remained significant when we introduced controls for mother's household income. The remaining four columns of the table present results from an analysis of covariance in which the income categories were treated as an unordered polytomy. For the two measures of problem behavior, the pattern of the coefficients corroborates the finding from the interval scoring: each increase in support yields an increasingly negative coefficient.

We consider these estimated effects on problem behavior to be the strongest evidence in our data of an influence by the nonresidential father. Still, there is no clear evidence of an effect of child support on other aspects of well-being. Again we searched for conditions that would amplify or reduce the importance of paternal participation. Marital status and income of the mother were obvious examples because child support payments might be most crucial

Table 2. Effects of Father's Support Payments on Child's Well-Being

	Interval $\beta$	Coefficients for ANCOVA Model <sup>a</sup>			<i>p</i>
		\$1- \$1,199	\$1,200- \$2,399	\$2,400 +	
<i>Mother's Report</i>					
Delinquency	-.12	.11	-.05	-.20	.22
Problem behavior	-.16*	-.21	-.28	-.38*	.16
Distress	.00	-.26	-.25	.12	.36
Academic difficulty	-.07	.39	-.27	-.11	.14
<i>Teacher's Report</i>					
Problem behavior	-.23**	-.13	-.46*	-.54*	.05*
Academic difficulty	-.07	-.32	-.42	-.04	.26
<i>Child's Report</i>					
Delinquency	-.06	-.01	-.26	-.09	.68
Dissatisfaction	.11	-.22	-.31	.23	.18
Distress	.00	-.24	.11	-.04	.76
Academic difficulty	.05	.32	-.06	.17	.49

Note: In addition to parental contact or closeness, the regressions included variables describing the child's age, race, sex, birth order, and region of residence, as well as the mother's education, religious preference, age at birth of the child, age at birth of first child, current marital status, foreign vs. U.S. birth, and timing marital dissolution.

<sup>a</sup> Each contrast is with children whose mother's report no paternal support during the preceding year.

\*  $p \leq .05$ .

\*\*  $p \leq .01$ .

when a family has only one breadwinner or when the financial situation is precarious. As before, we found no evidence of such interactive effects.

## DISCUSSION AND CONCLUSION

The general absence of effects of paternal participation on children's well-being is surprising in view of the widespread belief that children benefit from maintaining contact with their fathers. In addition, the effects of father's participation did not depend on the sex of the child or the presence of a stepfather. On the other hand, we did find some evidence that the level of child support is related to the incidence of problem behavior.

Do outside fathers really make no difference, except perhaps for the child support they provide? There are several possible explanations for these negative results. First, our relatively crude measures of well-being may not reveal subtle differences that are detectable using clinical or observational techniques. Moreover, none of our measures taps the area of sex-role patterns, which Hetherington et al. (1978, 1979) found to be most strongly influenced by paternal participation. Nevertheless, we found that closeness to the mother is related to our measures, and so are many other variables not examined in this paper.

Second, the level of paternal contact is so low in this national sample that there may be too few cases in the high-contact categories to produce statistically significant results. This may be one way to reconcile our results with available clinical studies. Nonresidential fathers in the Wallerstein and Kelly and Hetherington et al.

studies saw their children much more frequently than did the NSC fathers we studied. Perhaps if we compared children who never saw their father with a sizable sample of children who saw their father several times a week and had a deep and emotionally satisfying relationship with him, then we would see the effects of paternal contact. Although we cannot rule this out, the ANCOVA coefficient estimates not shown in Table 1 are not encouraging. Of the ten contrasts between the two groups with highest and lowest contact, nine are in the wrong direction; i.e., the children with high-contact fathers are doing more poorly than those who hadn't seen their fathers in five years. Still, we say little about the potential impact of truly involved fathers—those men who are deeply involved in raising their children. It remains for future research to explore this possibility.

On the other hand, results reported here are consistent with findings from other work that has traced the long-term consequences of adversity in childhood and adolescence. The subsequent life course of adolescents and young adults is exceedingly complex and there are many paths to recovery for those who experience stressful events such as teen pregnancy or the divorce of parents. These results challenge simple notions that single events, even major ones, set individuals on irreversible and unfavorable life course trajectories. As the life course unfolds, subsequent experience tends to dull the differences between those who experienced such events and those who did not.

The policy implications of findings reported here are unsettling because they clash with prevailing practice that attempts to increase paternal involvement. On the basis of our study,

we see no strong evidence that children will benefit from the judicial or legislative interventions that have been designed to promote paternal participation, apart from providing economic support. Of course these interventions could be justified on other grounds. If paternal contact involves some responsibilities and duties, this participation may ease the mother's childrearing burdens. Thus, mothers may benefit from paternal contact even if children do not (Longfellow 1979, p. 291). Likewise, fathers might benefit emotionally from contact with their children (Lamb and Sagi 1983). In short, our data suggest only that contact with fathers does not produce uniformly positive outcomes for children.

In summary, we do not advocate abandoning present efforts to involve noncustodial fathers. No single analysis or data source can provide an unqualified answer to the questions we raise, and firm conclusions must await further evidence. It would be premature to conclude that paternal contact has no or little influence. Our findings are a piece of evidence—we think an important piece—that should be considered with data from clinical sources in assessing the effect of paternal contact on children of divorced parents.

This topic surely merits more careful attention by researchers and policy makers. It is disconcerting to discover weak evidence for an almost commonplace assumption in popular and professional thinking—that children in disrupted families will do better when they maintain frequent contact with their fathers. In the absence of better and more convincing evidence, policy makers rely on conventional wisdom that is, unfortunately, an unreliable guide for social reform.

## APPENDIX

### *Component Items for Scales of Well-Being*

#### *Mother's Report*

##### *Delinquency* (alpha = .60)

1. Since January 1977, about the time of the first interview, has he/she had any behavior or discipline problems at school resulting in your receiving a note or being asked to come in and talk with the teacher or principal?
2. Has (child) been suspended, excluded, or expelled from school since January 1977?
3. Since January 1977, has (he/she) run away from home?
4. Since January 1977, has (child) stolen anything, regardless of its value?
5. How many times, if any, has (child) been stopped or questioned by the police or juvenile officers?

##### *Problem Behavior* (alpha = .69)

Tell me whether each (of the following) statement(s) has been . . . true of (child) during the past three months:

1. Cheats or tells lies.
2. Is disobedient at home.
3. Is disobedient at school.
4. Hangs around with kids who get into trouble.

##### *Distress* (alpha = .69)

Tell me whether each (of the following) statement(s) has been . . . true of (child) during the past three months:

1. Has sudden changes in mood or feelings.
2. Feels or complains that no one loves (him/her).
3. Is too fearful or anxious.
4. Feels worthless or inferior.
5. Is unhappy, sad, or distressed.

#### *Teacher's Report*

##### *Problem Behavior* (alpha = .79)

1. In your class, how often was any disciplinary action required for this student?

For each of the following statements, please indicate . . . how much like that this student was in 1980-81:

2. Fought too much, teased, picked on, or bullied other students.
3. Cheated, told lies, was deceitful.
4. Had a very strong temper, lost it easily.

##### *Academic Difficulty* (alpha = .95)

How did this student compare with others in his/her class last year (1980-1981)?

1. Verbal ability?
2. Math ability?
3. Overall performance?

#### *Child's Report*

##### *Delinquency* (alpha = .52)

1. How many times, if ever, have you been stopped or questioned by the police or juvenile officers about something they thought you did wrong?

In the last year, about how many times have you:

2. Hurt someone badly enough to need bandages or a doctor?
3. Lied to your parent(s) about something important?
4. Taken something from a store without paying for it?
5. Damaged school property on purpose?

##### *Dissatisfaction* (alpha = .71)

Are you satisfied, somewhat satisfied, or not too satisfied with:

1. Your friends?
2. Your family?
3. Yourself?
4. Being a (boy/girl)?
5. Being an American?

##### *Distress* (alpha = .46)

1. Do you feel lonely and wish you had more friends?
2. Do you have days when you are nervous, tense, or on edge?
3. Do you have days when you are unhappy, sad, or depressed?
4. All things considered, (how) is your life going?

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# RELATIVE COHORT SIZE AND YOUTH CRIME IN THE UNITED STATES, 1953-1984\*

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*This paper tests the cohort size hypothesis that crime rates fluctuate according to the relative size of the age (birth) cohort. The hypothesis is tested using arrest statistics of the Uniform Crime Reports for the years 1953-84. Statistics on the Index Crime rate and on the individual index crimes are included in the analysis. The age-period-cohort analysis reveals large age and period effects but small cohort effects. The statistical tests demonstrate that cohort size is not a good predictor of cohort variability. No cohort size effect emerges on the Index Crime Rate or in the individual index crimes. Thus, the data do not support the hypothesis. We suggest possible explanations for these contrary findings and speculate that the cohort size thesis is flawed on theoretical grounds.*

The purpose of this paper is to examine empirically the cohort size hypothesis: all else being equal, large youth cohorts will exhibit a relatively higher crime rate than small youth cohorts. Using arrest statistics of the FBI's *Uniform Crime Reports* for the years 1953-84 for the peak crime ages of 15-24, we ask, "Do children born in large cohorts and reaching adolescence from roughly 1962-75 have stronger delinquent tendencies than those born in other periods (i.e., those reaching adolescence from 1953-61 and from 1976-84)? Or, are there "delinquent generations" associated with birth cohort size?

## THE COHORT HYPOTHESIS

Criminologists believe that (a) involvement in crime diminishes with age; and (b) fluctuations in the age composition of a population can have a significant impact on the crime rates of that population.<sup>1</sup> In the United States, for example, both the dramatic increases in the overall crime rate in the 1960s and early 1970s and the decreases in the rate in the 1980s are linked to the post-World War II "baby boom" and the post-1960 "baby bust" (Steffensmeier and Harer 1987).

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<sup>1</sup> Due to space limitations, our review of prior research, discussion of statistical procedures, and presentation of findings are abbreviated. Readers interested in a more comprehensive treatment should contact the authors.

Recently, however, a number of demographers, such as Ryder (1965) and Easterlin (1978), have suggested that, in addition to a population's age pyramid, fluctuations in the size of birth cohorts can have a profound impact on the volume of crime in a society. In its simplest terms, this hypothesis suggests that, all else being equal, large youth cohorts will exhibit higher crime rates than small youth cohorts.

These demographers argue that cohort size affects both life opportunities and social development. In large cohorts, too many young people compete for jobs and education, and they feel the world is a less hospitable place than they were led to believe as children. Also, adults continually inculcate the young of each generation into the norms and values of adult society. As Ryder (1965, p. 845) puts it, "Society at large is faced perennially with an invasion of barbarians." Especially in a society already undergoing rapid social changes, the growth of large juvenile cohorts complicates adult society's attempt to reorient the self-interests of youth to the adult community interests (i.e., to civilize those barbarians) and encourages the development of youth subcultures and generational conflict.

In sum, social constraints and life opportunities will differ for abnormally large or small youth cohorts. While the social structure will eventually adapt to the changing population base, lags will develop during which time large cohorts will face fewer resources and more controls, and small cohorts are blessed with more riches and fewer constraints.

## PRIOR RESEARCH

Although criminologists have conducted cohort-based research (Wilkins 1960; Christiansen

1964; Slater, Darwin, and Ritchie 1966; Pullum 1977; Greenberg and Larkin 1985), they have not adequately addressed the effects of increased or decreased cohort size on crime. An exception is Maxim's (1985) test of the cohort size hypothesis, using juvenile court statistics on delinquency from the province of Ontario for the years 1952–81. He reports that a significant proportion of the variation in delinquency rates for ages 7–15 can be explained by variations in cohort size. As cohort size increased during the 1950s and early 1960s, the probability of one member of a cohort being sanctioned for delinquent behavior increased disproportionately to the size of his cohort.

Maxim's findings are provocative, but they must be treated with caution. First, he considered only the very young (ages 7–15) rather than late adolescents or young adults. The latter are most relevant to the cohort size hypothesis because official data show that rates of delinquency climb and then crest in the late teens or early 20s. Second, Maxim used juvenile court data subject to considerable bias because of shifts in law enforcement practices aimed at youth. Third, Maxim used a summary measure of crime—the *total number* of individuals appearing before juvenile court in a given year—which tends to be heavily weighted by the swamping effect of high-frequency, low-seriousness offenses. Also, the use of a global measure does not consider whether cohort size effects vary by type of offense. Finally, Maxim does not conduct a statistical test of the effects of cohort size on cohort criminality. Rather, his conclusion of a cohort size effect is based solely on a scatterplot of the cohort parameters against their population size. Our reading of the plot, however, suggests only a very weak effect at best.<sup>2</sup>

To test the cohort size hypothesis, we first decomposed the relevant data into age, period, and cohort effects. Then we examined the data to determine whether the cohort effects, if they exist, are largely influenced by the size of the cohort.

## METHODOLOGY

The FBI's *Uniform Crime Reports* records arrests by year of age for ages 15–24 in urban

areas only for the time period from 1953–84. Coverage does not extend to the rest of the U.S. until 1963. Reporting biases in the UCR arrest data are commonly acknowledged (Wolfgang 1963), but an age-period-cohort analysis of these data is no more subject to such biases than are the usual analyses of age-specific or period-specific arrest rates.

While the UCR provides arrest data on 27 categories of crime, we confine our analysis to the offenses that comprise the Crime Index: murder, aggravated assault, robbery, burglary, larceny-theft, and motor vehicle theft.<sup>3</sup> These are generally defined as the more serious crimes, and arrest data for the index crimes are more systematically gathered and less subject to fluctuation in police or reporting practices than nonindex crimes.

The relevant data were first decomposed into age, period, and cohort effects. Regarding the relative contributions of these three factors to the variance in the dependent variable, we expected a priori the age effects to be the largest and the cohort effects to be the smallest. Age has long been recognized as one of the most significant variables in predicting rates of official crime and delinquency. In constructing our model, therefore, we controlled for the impact of this variable prior to examining the effects of the other two.

Current research and theory on age, period, and cohort effects also suggest that period effects ought to have primacy over cohort effects (Hobcraft, Menken, and Preston 1982). Period effects are unique sociohistorical impacts that influence all age groups at a given time. These include changes in enforcement patterns or in economic conditions that might influence the type and volume of criminal behavior exhibited by a population. Fortunately, as we describe later, two of the three measures we used to test for cohort size effects are not dependent on whether period or cohort effects are given primacy.

## Estimation Procedures

The procedures we employed to test the cohort size hypothesis essentially replicate those used

<sup>2</sup> As this article was going to press, we became aware of an article by Smith (1986) who reports both a large cohort and a cohort size effect on homicide rates over the 1952–76 period. Smith's analysis and conclusions differ considerably from ours. However, we conclude that he neither distinguishes cohort effects from age and period effects nor does he provide a direct test of the cohort size hypothesis. A detailed discussion of Smith's study is available from the authors.

<sup>3</sup> For reasons of continuity, forcible rape is not included in our listing of index offenses because of a change in UCR reporting practices in 1958, when the classification of "rape" was redefined to include only forcible rape. Statutory rape, previously included in the "rape" category, was now classified under sex offenses. Also, should anyone wish to replicate our analysis, we discovered an error in the 1981 urban arrest table for the offenses of larceny-theft and burglary. Because of apparent typing or keypunching mistakes, the age-specific arrest tabulations for these two offenses are off by one column in the table.

by Maxim (1985). To specify and estimate age, period, and cohort effects, we used age-specific arrest rates as the dependent variable and analyzed the data using the conventional linear model approach (Hobcraft, Menken, and Preston 1982), with the following structural equation:

$$f(Y_{APC}) = b_o + \sum_i b_i A_i + \sum_j b_j P_j + \sum_k b_k C_k$$

In this model,  $Y_{APC}$  refers to age-period-cohort-specific rates;  $f$  if the logarithmic transformation of these rates; the  $b$ 's are standard regression weights, and  $A_i$  and  $P_j$  and  $C_k$  represent the  $i^{\text{th}}$ ,  $j^{\text{th}}$ , or  $k^{\text{th}}$  age, period, and cohort effects respectively.  $A_i$ ,  $P_j$ , and  $C_k$  are transformed into indicator variables through the use of dummy coding. For purposes of model identification, four restrictions are imposed on the model. (For a treatment of the identification problem in cohort analysis, see Mason, Mason, Winsborough, and Poole 1973; Clogg 1985.) The first three restrictions are similar to those typically used in dummy variable regression and are described in the following equation:

$$b_1^A = b_1^P = b_K^C = 0$$

where age 24 (oldest age) was chosen as the contrast level for age, 1953 (earliest period) as the contrast level for period, and cohort one (oldest cohort) was chosen as the contrast level for cohort. The fourth restriction is in the form  $b_1^A = b_2^C$ , which sets the effects of the second oldest cohort equal to the effects of the oldest cohort. Based on a priori knowledge of the cohorts studied here, we equate the two oldest cohorts because (1) these cohorts are about equal in size (both absolute and relative); (2) they were born prior to the time (roughly 1940) after which Easterlin hypothesized that variable cohort size should have an effect on crime rates; (3) they were born and reached adolescence during a period characterized by cultural and social stability, and, thus, would have shared similar life experiences; and (4) it is reasonable to assume that the arrest data for the oldest cohorts are the least valid and reliable.

### Relative Cohort Size

Relative cohort size is defined as the proportion of young (15–24) to older (25–64) in society. This ratio increased from 0.27 in 1953 to 0.41 in 1975, and then decreased to 0.34 in 1984. Increases in this ratio are said to (1) depress starting salaries and make it difficult for youths to obtain employment suitable to their educational credentials (Easterlin 1978); and (2)

reduce the effectiveness of adult authority figures (which exclude the very young and the very old) to constrain or "civilize" the youth (Ryder 1965).

### Relative Criminality of Youth Cohort

To help explain the relative importance of the three main groups of effects (age, period, and cohort) and, in particular, to help assess the cohort size effect, we calculated two measures of the relative criminality of the youth ages. First, we calculated for each year of the 1953–84 period the *proportionate age involvement* (PAI) in crime for all ages. This measure adjusts for the percentage of people in different age groups, and it represents a cumulative percentage. Summing across all age groups, it equals 100 percent. As such, PAI is an easily interpreted and demographically sensitive measure of change that provides a robust comparison of crime levels across age groups and easy identification of shifts in the criminality of youth compared to other age groups in the society as a whole (Steffensmeier 1987). The formula for PAI is

$$PAI = \frac{R_{ij}(100)}{\sum_i R_{ij}} \quad \frac{R_{ij}(100)}{\sum_i R_{ij}}$$

where  $i$  = age category (under 14, 15, 16, ..., 25–29, ..., 50 and over)

$j$  = offense category (index rate, homicide, etc.)

$R$  = rate/100,000  $\times$  number of age groups in the  $i$  age category.

Second, we calculated a related or subset measure of PAI, the *proportionate age involvement in youth crime* (PAI-YC). This measures the relative criminality of each youth age compared to other youth and is derived by summing across the ages 15–24. The formula for PAI-YC is

$$PAI-YC = \frac{R_{ij}(100)}{\sum_i R_{ij}}$$

where  $i$  = age category (15, 16, ..., 24)

$j$  = offense category (index rate, homicide, etc.)

$R$  = rate/100,000.

## FINDINGS

### Cohort Size Effects for the Crime Index

Table 1 summarizes the proportion of variation in youth arrest rates for the Crime Index (i.e., total of six index offenses) by various age,



Table 1. Proportion of Variance in Youth (15-24) Index Rates Explained by Various Age, Period, and Cohort Models

Model	Number of Parameters	R	R <sup>2</sup>
A	9	.8510	.7242
AP	40	.9887	.9775
AC	49	.9772	.9550
APC	80	.9960	.9921

Notes: Total observations (cells) = 320. Variables coded in models are A = all age parameters, P = all period parameters, and C = all cohort parameters.

period, and cohort models. The full APC model produces R<sup>2</sup> values of .9921, indicating that the fully specified model fits the data extremely well.

Age alone accounts for 72 percent of the variation. Adding period effects to the "age only" model increases the proportion of variance explained by 25 percent (from .7242 to .9775). The further addition of cohort effects adds only 1.5 percent in explained variance to the age-period model (from .9775 to .9921).

Table 2 summarizes the extent to which period and cohort effects explain residual variation in the index rate (i.e., once age is controlled). As the first row in Table 2 shows, about 28 percent of the variation in rates is unexplained. Period effects alone account for 92 percent of the residual variation in rates. On the other hand, cohort effects alone explain 84 percent of the residual variation. These values are somewhat misleading because period and cohort effects are not totally independent but share some variance.

An extended way of determining the unique contribution of cohort effects is to weigh their contribution to the residual variation once both age and period are controlled. This is a stagewise regression procedure that allows the variables to be entered in such a way that the expected direction of any effects can be "preserved" when there is a high degree of collinearity among the independent variables

(Draper 1981).<sup>4</sup> First, we fitted a model including only age and period effects to the data. The estimated parameters for the age-period model are displayed in Appendix A. The distribution of parameters reveals a steady decrease in rates with age, and a general increase in rates over the time span.

Second, after the age and period effects were calculated, we estimated cohort parameters by regressing the residuals from the age-period model on the cohort design matrix (see Appendix A). The cohort effects are small, with only a few of the effects statistically significant. Moreover, there do not appear to be any clear-cut patterns in the effects.

### Cohort Size Effects

Because our primary purpose was to test the cohort size hypothesis, the next step was to determine whether the small cohort effect observed in the data is that of cohort size. Recall that the hypothesis predicts that the relative criminality of youth will be greater with larger cohorts.

We employed alternative procedures to test this prediction, including the PAI/PAI-YC measures. The PAI defines the criminal involvement of a specific youth age relative to the (total) criminality of all other ages in society, while the PAI-YC defines the criminal involvement of a specific youth age relative to total youth criminality (i.e., ages 15-24). When the PAI-YC is summed over the individual years of a cohort's life (i.e., across the diagonals of a conventional cohort table) it equals 100 percent if there are no cohort effects whatsoever. If there are departures from the norm of 100 percent, then it can be inferred that the period effects are different for different age groups and cohorts, or that there are important interactions between age, period, and cohort. A PAI-YC value larger than 100 percent indicates greater relative criminality of that cohort, while a value less than 100 percent indicates smaller relative

Table 2. Proportion of Residual Variation (Controlling for Age) Explained by Period and Cohort Effects

R <sup>2</sup> Partition	Variance Explained
1 - A	.2758
AP - A	.9184
1 - A	
AC - A	.8368
1 - A	
APC - A	.9714
1 - A	

Notes: See Table 1 for R<sup>2</sup> values. Variables included in the partitions are A = age effects, P = period effects, and C = cohort effects.

<sup>4</sup> A straightforward least squares equation does not always achieve this on account of the correlations among the independent variables. In the analysis here, there is extreme multicollinearity among the independent variables *age*, *period*, and *cohort*, so that we are unable to isolate the specific effects of cohort in the full APC model. Also, the standard errors in the full model are very high. Finally, we found that the cohort parameters in the full model are extremely vulnerable to the particular fourth restriction imposed on the model (see estimation procedure). For instance, different restrictions produced very different cohort effects, in either a positive or negative direction. However, when the stagewise regression procedure is used, this "problem" is not encountered. That is, the cohort effect is the same regardless of the restrictions imposed on the model.

Table 3. Regression of Cohort Size on Cohort Criminality (Cohort Parameters, PAI-YC, PAI) for Index Rate and for Individual Index Offenses

	Regression Coefficient		
	Cohort Parameters	PAI-YC	PAI
Index rate	-.0236	-2.137	0.401
Homicide	-.0247	-2.385	5.200*
Robbery	-.0462*	-4.586*	1.090
Assault	.0110	0.026	4.640*
Burglary	-.0634**	6.117*	-0.367
Larceny	-.0033	-0.418	1.790
Auto theft	-.0122	-0.489	-3.610*

\*  $p < .05$ .

\*\*  $p < .01$ .

criminality. For PAI, the same logic applies, except that the grand mean (summing across all birth cohorts and dividing by the number of cohorts) is used as the norm.

First, we regressed both the estimated cohort parameters and the sums of both PAI-YC and PAI of the cohorts on relative cohort size. The results are summarized in Table 3, which also displays the findings for the individual index offenses that are examined later. For the Index Crime Rate, cohort size does not have a significant effect on the relative criminality of cohorts. In fact, the relationship is negative (although nonsignificant) for two of the three measures of cohort criminality, with cohort criminality actually *decreasing* slightly with cohort size. It is worth repeating, furthermore, that while the cohort parameters are based solely on that variation unexplained by age and period, the PAI/PAI-YC do not assume any order in primacy.

Second, in Appendix B we list the cohorts ranked according to their relative size, along with their corresponding cohort parameters, their PAI-YCs, and their PAIs. As the distribution of those PAI-YCs and PAIs across cohorts shows, there is no pattern whatsoever to the minor departures from the norm, defined as 100 percent for PAI-YC and 49 percent for PAI. If there were a pattern, or if the departures were large, this would suggest a cohort size effect. Instead, the departures are small, more or less in the order of random fluctuations around a common mean. More importantly, the directionality of the departures (e.g., whether the PAI-YCs fall above or below 100 percent) is distributed equally not only between large and small cohorts but also between *abnormally* large and small cohorts. That is, cohort size is unrelated to, or has no ability to predict, the relative criminality of a cohort over its youth life.<sup>5</sup>

<sup>5</sup> Several researchers have noted a dip or a lower crime rate for the 1947 birth cohort (Blumstein, Cohen, and Rosenfield 1986), and suggest that this is due to the

## COHORT SIZE EFFECTS FOR INDIVIDUAL INDEX CRIMES

So far, we have seen that the cohort size hypothesis is not supported by the data on the Crime Index. Next, we examined whether a cohort size effect exists for any of the individual crimes of the Crime Index. Because the components of a global measure like the Crime Index may be heavily weighted by arrests for a single offense (e.g., larceny-theft) or by other causes that may reflect different patterns, the use of the Crime Index may mask important cohort size effects for some of the individual index crimes. To check this, we employed the same procedures used above to test for age-period-cohort effects and for cohort size effects on the individual index crimes. We summarize the major results in Tables 3 and 4.

### Age-Period-Cohort Effects

For homicide, age alone explains 55 percent of the variation in rates. (See Table 4.) There is no clear-cut peak age for homicide, but the rates rise until about age 17, remain fairly constant until the mid-20s, and then decline rapidly. The addition of period effects to the "age-only" model increases the proportion of variance by 40 percent (from .5492 to .9527). The period effects show a general increase in homicide rates until the mid-1970s, then a leveling off. The further addition of cohort effects increases the explained variance by only 2 percent (from .9527 to .9717).<sup>6</sup>

For the remaining index offenses, we find either large age effects or large period effects with only small cohort effects. (See Table 4.) Aggravated assault rates peak at ages 18–24, and robbery rates at age 17, after which both offenses decline steadily. However, the decline is not as steep as for the other index crimes. Burglary, larceny-theft, and auto theft rates peak at age 16, after which the rates for all three

disproportionate number of firstborns in 1947. Supposedly, the oldest child tends to be more conforming or less crime prone than younger siblings. Rosenthal (1980) has demonstrated, however, that the dip is essentially a statistical artifact resulting from the particular timing of the start of the baby boom, combined with differences between the UCR, in its annual count of age-specific arrests, and the Census Bureau, in its annual count of age-specific population figures. In light of Rosenthal's findings, we replicated our analysis by omitting the 1947 cohort. The results were comparable to those reported in this paper.

<sup>6</sup> In terms of the residual or the non-age attributable variance, the contribution of cohort effects (once period is controlled) is 4 percent for homicide. For the other index offenses, it is 8 percent for aggravated assault, 13 percent for robbery, 13 percent for burglary, 4 percent for larceny, and 28 percent for auto theft.

Table 4. Proportion of Variance in rates Explained by Various Age, Period, and Cohort Models and Proportion of Residual Variation (Controlling for Age) Explained by Period and Cohort Effects

	$R^{2a}$			$R^2$ Partition <sup>b</sup>		
	A	AP	APC	AP-A 1-A	APC-A 1-A	APC-AP 1-A
Homicide	.5492	.9527	.9717	.8951	.9372	.0421
Robbery	.2304	.8729	.9750	.8348	.9675	.1327
Assault	.2223	.9211	.9849	.8985	.9806	.0821
Burglary	.8460	.9716	.9915	.8156	.9448	.1292
Larceny	.6115	.9788	.9938	.9454	.9840	.0386
Auto theft	.8907	.9645	.9951	.6752	.9552	.2800

<sup>a</sup> Proportion of all variance explained by age, period, and cohort effects.

<sup>b</sup> Proportion of residual variation (controlling for age) explained by period and cohort effects.

offenses, but especially for auto theft and burglary, decline sharply. The addition of cohort effects (to the age-period model) increases the explained variance by 10 percent for robbery, 6 percent for aggravated assault, 3 percent for auto theft, and 2 percent for both burglary and larceny-theft.

#### Cohort Size Effects

The results of our analysis of cohort size effects are summarized in Table 3. There is not much evidence of a cohort size effect in the data on the individual index crimes. When the estimated cohort parameters are regressed on relative cohort size, the results show nonsignificant *b*'s for homicide, assault, larceny-theft, and auto theft but *negative* significant *b*'s for robbery and burglary. Overall, cohort involvement in the index crimes (especially robbery and burglary) actually decreased with greater cohort size. Similar results are reported when PAI-YC is regressed on cohort size. (See the middle column of Table 3.)

The findings are less clear-cut when PAI is regressed on cohort size. The relationships are negative for burglary and auto theft, but they are positive for the other index offenses. The positive coefficients for homicide and assault are significant and provide the only evidence in the statistical analysis that supports the cohort size hypothesis. This may simply reflect a chance finding; that is, we could expect one or two of the twenty-one regressions in Table 3 to be positively significant. Or this may reflect the greater instability of the PAI measure, because it involves the proportionate criminal involvement of all ages rather than only the youth ages.

#### SUMMARY AND IMPLICATIONS

In this report, we analyzed crime rate trends to answer two questions: (1) Do different youth cohorts have different levels of "intrinsic" risk of criminal involvement "net" of age and period? and (2) Is cohort size a predictor of cohort variability in criminal involvement?

First, the statistics on the Index Crime Rate and the individual index offenses showed large age and period effects but small cohort effects. Second, cohort size was not a good predictor of that cohort variability that did exist. There was no cohort size effect on the Index Crime Rate and no clear-cut cohort size effects on the individual index crimes. If a discernible pattern existed, it was opposite to the cohort size hypothesis. Overall, larger cohorts were slightly *less* criminally involved than smaller ones. Thus, the findings of this report are inconsistent with the hypothesis that a larger cohort means higher youth crime rates. It is important, however, to regard our results with caution. We will discuss four possible explanations of the findings.

#### Measurement Problems in "Official" Crime Data

The "official" statistics we used refer to arrests made and reported by individual police agencies throughout the nation. As criminologists have long recognized, "delinquency" or "crime" is the product of a complex interaction between individual behaviors and societal responses. Rates of official delinquency/crime are moderated by the limited capacity of social control agencies to respond to delinquent behavior. Agencies may become saturated. Thus, there may be a leveling, or even a drop, in the official delinquency rates as the rate of growth in youth criminality surpasses the rate of growth in the social control response mechanisms. Conversely, we would also expect a certain amount of "stickiness" in rates in situations where cohort size is generally declining.

While plausible, this interpretation is unlikely because arrest rates rise and fall considerably over the examined time period. The stickiness argument would be applicable only if we assume that the police somehow manage, either intentionally or otherwise, to maintain consistency in the arrest ratio of one age group with another. Also, the position that crime statistics are inadequate ignores both (a) the consistency of the findings, that is, the overall absence of

cohort size effects across *all* the index offenses; and (b) the absence of a cohort size effect on rates of homicide, a crime not much affected by enforcement or reporting bias.

*Time Span for Analysis Is Too Short; Cohort Size Effect May Still Materialize*

A second explanation of why our findings are inconsistent with the cohort size hypothesis is that the analysis does not cover a long enough time span for testing the hypothesis, particularly since we have incomplete data on the shrinking youth cohorts entering mid-adolescence in the late 70s. Because they are incomplete, data on these smaller cohorts were not included in our analysis. There appears to be some decline in relative criminality among these most recent youth cohorts (e.g., the 1984 arrest rates and PAIs of 15-, 16-, and 17-year olds are smaller than the 1979 rates). If this trend were to hold as these incomplete "baby-bust" cohorts pass through the highest-crime ages (15-24), evidence of a cohort size effect would be ascertainable by the early 1990s.

This interesting speculation is only weakly suggested by the data. Also, the cohort size hypothesis predicts that abnormally large youth cohorts will exhibit high relative criminality. This prediction is clearly not supported by the data.

*Substitution Effect*

A third plausible explanation of the failure to find a cohort size effect is that cohort size interacts, in ways unknown, with police practices and crime opportunities so that prospective offenders in large cohorts turn away from the index crimes but commit nonindex crimes at a higher rate. This, in part, may happen because of the more severe sanctioning of the "rapidly rising" index ("serious") crimes that coincide with the growth in the proportion of youth in the population at large. Members of abnormally large cohorts may be committing more nonindex crimes, such as drug dealing and vandalism, and avoiding index crimes because of stronger deterrents for the latter. The former are less severely sanctioned and entail less risk of arrest or imprisonment.

*Cohort Size Argument Is Theoretically Flawed*

Fourth, it may be that the cohort size position is flawed on theoretical grounds, and that we should *not* expect an association between cohort size and criminal behavior. The hypothesis presupposes that larger relative cohort size means only one thing—more crime. But the assumption of a simple linear relationship between cohort size

and relative youth criminality ignores other material and structural changes that may also accompany the cohort phenomenon.

For example, growth in the youth population mass is likely to be accompanied by a reciprocal response from the social system to increase the formal and informal deterrents to crime. Community-sponsored youth activities and other outer constraints may quickly mobilize as the system prepares to deal with a larger mass of young people. The expansion of these constraint mechanisms may diminish cohort insulation and the youth culture it engenders, and consequently, check or even reduce an increase in crime.

It may be that the diminished job opportunities faced by abnormally large youth cohorts foster adult-oriented and conformist expectations and attitudes, such as "One must work hard for what he gets," "When the going gets tough, the tough get going," or "Be prepared, lest one miss the opportunity." In contrast, small cohorts may be described as "pampered and spoiled" or "having things too easy." Broad structural changes seldom affect all population subgroups identically; an increase in the relative alienation and deprivation of some subgroups (because of declining job opportunities) may juxtapose with an increased commitment in other subgroups to a conventional work ethic as a prerequisite for getting ahead. In sum, some effects of a growth in cohort size may be criminogenic, while others may not be, so that their net balance will be offsetting or even tipped toward less crime. Also, the cohort size hypothesis ignores factors, such as income inequality and access to crime opportunities, whose effects on changes in crime rates may be so large that they nullify the small, would-be effects of cohort size on crime rates.

*Future Research*

There are several ways to extend the present research. First our research was limited to the index crimes. The framework and procedures we employed, especially the PAI/PAI-YC measures used to test for cohort size effects, should be applied to the nonindex crimes. Additionally, research is needed on other forms of deviance, such as suicide and alcoholism, whose rates over time are also thought to be affected by cohort size (Easterlin 1978).

Second, the model used in our research allows for main effects of age, period, and cohort, but it does not allow for any interactions among these variables. Subsequent research should consider incorporating interactions in the model (Clogg 1982). Post-World War II changes in education and in the labor force have increased the separation between adolescents and adults and made age boundaries more

distinct (Foner 1982). One might expect a kind of continuous "period shock" resulting in a general decline in the peak age of crime, and greater concentration of adolescent offenses (i.e., an age-period interaction). This also appears to be consistent with trends in age-crime curves since World War II (Steffensmeier, Allan, and Harer 1987).

Third, our research was limited to ages 15–24 because arrest data for individual years are not available for the other ages. Subsequent research should consider an interpolation of the five-year groupings to generate arrest statistics for the remaining ages. This not only would allow for inclusion of a larger number of complete cohorts but also would allow for an age-period-cohort (and cohort size) analysis covering a wider life span.

Fourth, this study was primarily demographic. We make no claim of having unraveled the causal mechanisms that have produced recent trends. With respect specifically to the cohort size hypothesis, future research needs to embed cohort size within a multivariate mode of crime in which the independent and joint effects of cohort size and other social-structural variables are assessed simultaneously.

Finally, we recognize the considerable disagreement among analysts regarding the choice of techniques for conducting an age-period-cohort analysis (e.g., in the choice/application of equality constraints). As Mason et al. (1973) demonstrate, cohort analysis performed without prior knowledge or strong theoretical preconceptions about which parameters are identical is subject to interpretation errors. The estimates derived from different cohort models can be quite distinct, so that different models are likely to produce different conclusions. Nonetheless, based on our choice of techniques and theoretical assumptions, we conclude that variability in cohort size is not a useful predictor of levels of youth criminality.

Appendix A. Age, Period, and Cohort Effects

Age Effects		Period Effects		Cohort Effects	
Age	Effect	Period	Effect	Cohort	Effect
15	1.012	1953	*	1	*
16	1.072	1954	.021	2	*
17	.930	1955	.038	3	.154
18	.756	1956	.163	4	.145
19	.546	1957	.271	5	.135
20	.358	1958	.189	6	.159
21	.287	1959	.180	7	.136
22	.189	1960	.022	8	.141
23	.097	1961	.092	9	.155
24	*	1962	.057	10	.166
		1963	.087	11	.218
		1964	.153	12	.271
		1965	.185	13	.254
		1966	.145	14	.219
		1967	.235	15	.142
		1968	.328	16	.174
		1969	.394	17	.181
		1970	.486	18	.245
		1971	.532	19	.081
		1972	.458	20	.188
		1973	.433	21	.209
		1974	.444	22	.253
		1975	.561	23	.234
		1976	.417	24	.195
		1977	.512	25	.184
		1978	.516	26	.179
		1979	.544	27	.176
		1980	.796	28	.192
		1981	.599	29	.193
		1982	.689	30	.198
		1983	.616	31	.211
		1984	.555	32	.237
				33	.226
				34	.222
				35	.211
				36	.179
				37	.142
				38	.111
				39	.035
				40	.007
				41	.003

\* Excluded from equation as equality constraint.

Appendix B. Full Cohorts Rank-ordered by Relatives Size with Corresponding Cohort Parameters and PAI-YC and PAI Summed Across Cohorts for Index Crimes

Cohort	Birth Year	Cohort Parameter	PAI-YC	PAI
10	1938	.166	96.3	45.2
11	1939	.218	102.4	48.2
12	1940	.271	108.6	51.8
13	1941	.254	107.6	51.7
14	1942	.219	103.6	49.7
18	1946	.245	104.9	49.7
17	1945	.181	98.3	46.7
16	1944	.175	98.6	46.7
15	1943	.142	95.6	45.7
22	1950	.253	106.4	51.0
21	1949	.209	102.0	48.5
20	1948	.188	99.9	47.3
23	1951	.234	105.0	51.0
32	1960	.237	104.4	51.5
24	1952	.195	101.1	49.7
25	1953	.184	99.8	49.5
31	1959	.211	101.8	50.6
26	1954	.179	99.3	49.4
28	1956	.192	100.1	50.1
27	1955	.176	98.7	49.1
30	1958	.198	100.5	50.4
29	1957	.193	100.7	50.2
19	1947	.081	90.0	42.6
			$\bar{X} = 49.0$	

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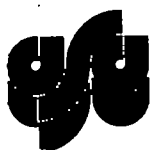
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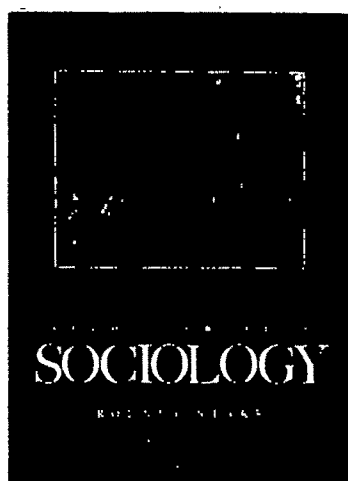
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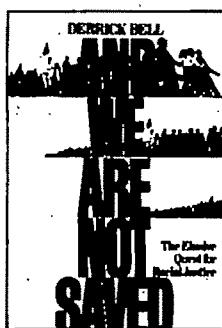
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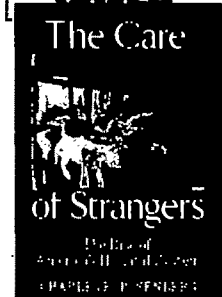


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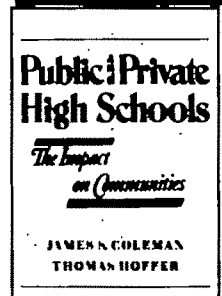


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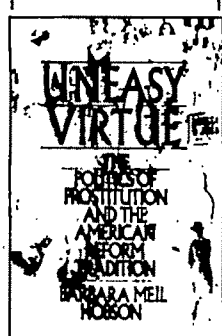
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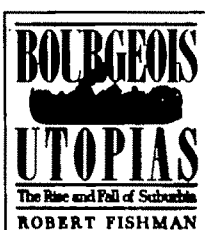


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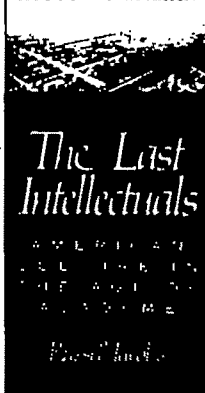
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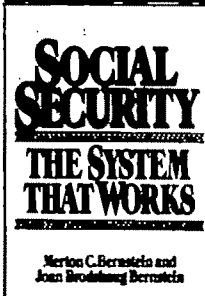
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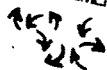


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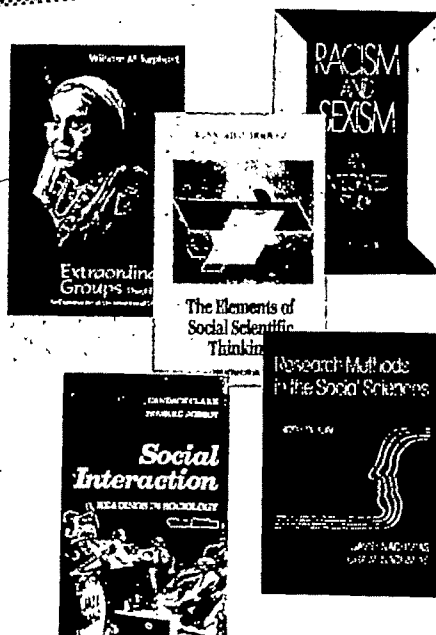
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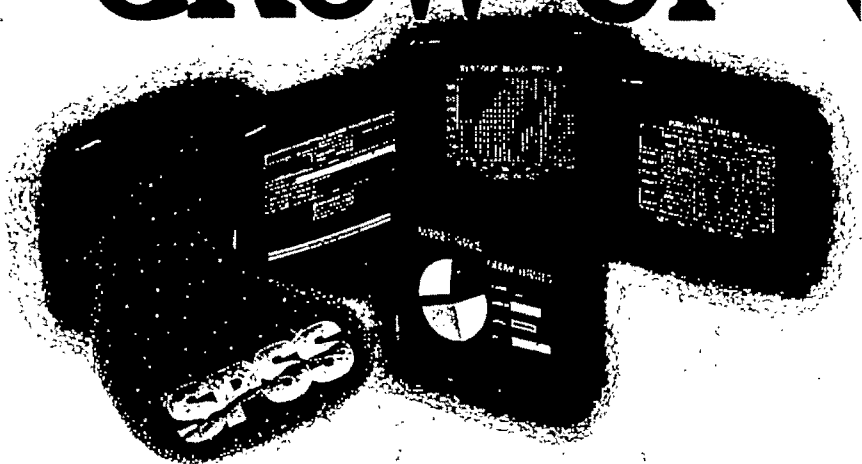
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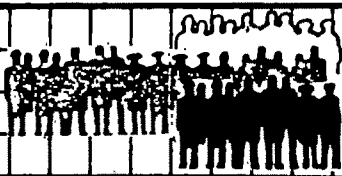
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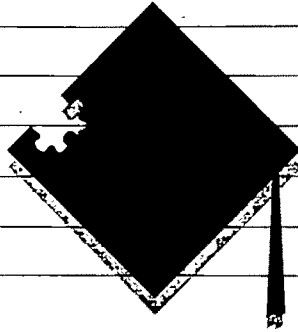
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Volume 52 • December 1987 • Number 6

**Melvin L. Kohn**

Cross-National Research as an Analytic Strategy

**Val Burris**

Business Political Partisanship

**Jimmy M. Sanders and Victor Nee**

Limits of Ethnic Solidarity

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Comment

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**Research Note**

**Linda Grant, Kathryn B. Ward, and Xue Lan Rong**

Gender and Methods in Sociological Research

**Official Journal of the American Sociological Association**

The *AMERICAN SOCIOLOGICAL REVIEW* (ISSN 0003-1224) is published bimonthly in February, April, June, August, October, and December by the American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036. Phone (202) 833-3410. *ASR* is printed by Boyd Printing Company, Albany, New York. Second class postage paid at Washington, D.C. and additional mailing offices. *POSTMASTER*: Send address changes to *The American Sociological Review*, 1722 N Street, N.W., Washington, D.C. 20036.

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# AMERICAN SOCIOLOGICAL REVIEW

December 1987

Vol. 52, No. 6

Contributors to this issue .....	iv
Guest Editorial .....	vi

## ARTICLES

Cross-National Research as an Analytic Strategy.....	<i>Melvin L. Kohn</i>	713
The Political Partisanship of American Business: A Study of Corporate Political Action Committees .....	<i>Val Burris</i>	732
Limits of Ethnic Solidarity in the Enclave Economy.....	<i>Jimmy M. Sanders and Victor Nee</i>	745
What's an Ethnic Enclave? The Case for Conceptual Clarity .....	<i>A Comment by Alejandro Portes and Leif Jensen</i>	768
On Testing the Enclave-Economy Hypothesis ..	<i>A Reply by Victor Nee and Jimmy Sanders</i>	771
The Formation of Justice Norms .....	<i>John F. Stolte</i>	774
Disorder in the Life Course: How Common and Does It Matter? .....	<i>Ronald R. Rindfuss, C. Gray Swicegood, and Rachel A. Rosenfeld</i>	785
Trends in the Residential Segregation of Blacks, Hispanics, and Asians: 1970-1980.....	<i>Douglas S. Massey and Nancy A. Denton</i>	802
Race, Family Structure, and Delinquency: A Test of Differential Association and Social Control Theories .....	<i>Ross L. Matsueda and Karen Heimer</i>	826
Initiation of Coitus in Early Adolescence .....	<i>J. Richard Udry and John O. G. Billy</i>	841

## RESEARCH NOTES

Is There an Association between Gender and Methods in Sociological Research? .....	<i>Linda Grant, Kathryn B. Ward, and Xue Lan Rong</i>	856
--	---	-----

## ASA DEPARTMENTS

Reviewers for the 1987 Issue.....	863
1987 Index, Volume 52, Number 1-6 .....	866
ASA Journals .....	870

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## CONTRIBUTORS TO THIS ISSUE

■ MELVIN L. KOHN (Cross-National Research as an Analytic Strategy), Professor of Sociology, Johns Hopkins University, continues his cross-national efforts in two ways: he is editing a volume on cross-national research in sociology for the ASA Presidential series, and he is writing a book (in collaboration with Kazimierz M. Slomczynski) on the results of their comparative analyses of social structure and personality in Poland and the United States.

■ VAL BURRIS (The Political Partisanship of American Business: A Study of Corporate Political Action Committees) is Associate Professor of Sociology, University of Oregon. His current research is on the politics of U.S. corporations, intercorporate networks, and business support for the New Right. He has also written extensively on theories of class structure and the politics of the new middle class.

■ JIMMY M. SANDERS (Limits of Ethnic Solidarity in the Enclave Economy) is Assistant Professor of Sociology, University of South Carolina. He studies the political character of the business cycle and state policies of income redistribution. With Victor Nee, he is exploring alternative modes of socioeconomic mobility among contemporary immigrant groups. VICTOR NEE is Associate Professor of Sociology, Cornell University. He is analyzing survey research data on peasant's market structures and state policy in China. He has recently completed a comparative study of state socialism in China and Eastern Europe with David Stark.

■ JOHN F. STOLTE (The Formation of Justice Norms) is Associate Professor of Sociology, Northern Illinois University. He is pursuing a cumulative line of experimental research on structural exchange theory and distributive justice. Recent publications include "Legitimacy, Justice, and Productive Exchange" in *Social Exchange Theory* edited by Karen Cook (Sage, 1987) and "The Legitimation of Structural Inequality" (*American Sociological Review*, 1983).

■ RONALD R. RINDFUSS (Disorder in the Life Course: How Common and Does It Matter) is Professor of Sociology, University of North Carolina. He is an author of the forthcoming monograph, *First Births in America: Changes in the Timing of Parenthood*. Current research focuses on antecedents of out-of-wedlock childbearing, adolescent fertility, the changing institution of marriage, and the family in Asia. C. GRAY SWICEGOOD is Assistant Professor of

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■ DOUGLAS S. MASSEY (Trends in the Residential Segregation of Blacks, Hispanics, and Asians: 1970-1980) is Professor of Sociology and Director of the Population Research Center at the University of Chicago. His book on Mexican migration to the United States, entitled *Return to Aztlan*, has just been published by the University of California Press. He is presently at work on a project examining the consequences of segregation for minority groups in the United States, in collaboration with Nancy A. Denton. NANCY A. DENTON is a Research Associate of the Population Research Center at the University of Chicago. She has published two earlier articles on racial and ethnic segregation, and has recently completed a study of segregation patterns among the elderly in U.S. cities. She is currently conducting research on the consequences of segregation for minority groups, in collaboration with Douglas S. Massey.

■ ROSS L. MATSUEDA (Race, Family Structure, and Delinquency: A Test of Differential Association and Social Control Theories), Assistant Professor of Sociology, University of Wisconsin-Madison, is currently engaged in three lines of research: developing and testing a structural symbolic interactionist theory of delinquency (with Karen Heimer); examining the relationships among social status, rational choice, and deterrence (with Irving Piliavin and Rosemary Gartner); and exploring the implications of statistical power for structural equation models (with William T. Bielby). KAREN HEIMER is a Ph.D. candidate in Sociology at the University of Wisconsin-Madison. Her

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■ LINDA GRANT (Is There an Association between Gender and Methods in Sociological

Research?) is Assistant Professor of Sociology and Faculty Associate in the Institute for Behavioral Research at the University of Georgia. She currently is conducting research on career choices and work and family balance among women and men physicians in the early career years and editing a special issue of *The Elementary School Journal* on minorities and education. KATHRYN B. WARD is Associate Professor of Sociology at Southern Illinois University at Carbondale. Her other research focuses on women in the global economy and the contemporary U.S. women's movement (with Rachel Rosenfeld). She is author of *Women in the World-System: Its Impact on Status and Fertility* (Praeger, 1984). XUE LAN RONG, a Ph.D. candidate in Social Science Education at the University of Georgia, is completing a dissertation entitled "Migration and Education in America, 1880-1980." She holds an undergraduate degree from Peking Teachers' University. She also is conducting research on homicide rates and economic conditions in the United States.

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## GUEST EDITORIAL

### SHARING DATA: IT'S TIME FOR ASA JOURNALS TO FOLLOW THE FOLKWAYS OF A SCIENTIFIC SOCIOLOGY

*Below is the first in a series of guest editorials that discuss important ASA publications issues. The editorial does not represent official ASR policy because that is made by the Publications Committee. Future issues of ASR will present alternative views on data sharing. Ventilation of various positions should stimulate readers and authors to arrive at a reasoned position. Ed*

These thoughts were prompted by the apparent reduction in type size between the June and August 1986 issues of the *American Sociological Review*. My eyesight is not very good, but it is no worse, I am sure, than that of many ASA members over 40; I wear trifocals. I found it extremely difficult to work my way through just one non-technical paper in the August 1986 issue, and I gave up on the rest. If ASR and other association journals are to serve as media of scientific communication, they ought to be legible.

Among colleagues with whom I spoke at the New York ASA meetings, there was strong sentiment to restore legible type size and spacing, even if this entails a reduction in editorial content. But that may not be necessary. It should be possible to maintain and improve the quality of scholarly and scientific communication in ASA journals and restore legible text by producing supplementary materials on a magnetic or other machine-readable medium, e.g., floppy diskette.

Sociologists, like other scientists, ought to share research data. The advantages—and potential problems—of data sharing and several proposals to improve and increase data sharing are laid out very nicely in the first 36 pages of a recent NAS-NRC Report (Fienberg, Martin, and Straf 1985). The report lists the following benefits of data sharing: reinforcement of open scientific inquiry; the verification, refutation, or refinement of original results; the promotion of new research through existing data; encouragement of more appropriate use of empirical data in policy formulation and evaluation; improvement of measurement and data collection methods; development of theoretical knowledge and knowledge of analytic techniques; encouragement of multiple perspectives; provision of resources for training in research; and protection against faulty data. The report finds that technical obstacles to data sharing, such as incompatibilities in machine and software systems and data file structures, are not as serious as they once were. The major obstacles to data sharing are the costs to the original researchers of providing adequate documentation and mechanisms

of dissemination, and those to later researchers of accessing the data.

Anyone who has experience with the secondary analysis of a large or complex body of survey data knows that there is a great deal of difference between “public” and “usable.” A classic, but little known, example is that of the famous Glass table of intergenerational mobility in post-war Great Britain, which has been the subject of so much empirical and methodological study. When the Hollerith cards from the study were recovered, it proved impossible to verify the original frequencies. Readers who doubt the continuing seriousness of this problem would do well to read DeWald, Thursby, and Anderson (1986). In a major journal of economics, where much of the published work is based upon nominally public data series, “inadvertent errors in published empirical articles are a commonplace rather than a rare occurrence” (p. 587). The National Science Foundation has recently required that data created in research projects that it supports must be made public in timely fashion. This is admirable, especially if the injunction is buttressed with sufficient support for documentation and dissemination. But there is a gap between the distribution of public data sources and the accessibility of working data files where, I believe, sociological researchers and journals have both an opportunity and an obligation to develop better mechanisms for data sharing.

At present, data sharing occurs haphazardly in ASA journals. There are no standard procedures for making data available to readers. There is great variation in the degree to which data are presented in papers or appendices in the form of a correlation matrix with means or standard deviations or a series of tables of cross-classified counts; in the presentation of documentation within a paper or appendix; or in the offer of data in machine-readable form from the authors. Editors often ask that I eliminate data or documentation, to keep the total size of a submission at a reasonable level. My experience as a reader, however, is that there is no standard practice in ASA journals and that data required

to replicate or test a finding could often be presented in minimal space, but have been left out.

Just to make it clear that my record is not clean on this score, I confess that I recently discarded a large stack of computer output, more than a dozen years old, in which five intergenerational mobility classifications, dating from 1947 to 1972, were compared (Hauser, Koffel, Travis, and Dickinson 1975). Just a day later, I received my first request for the original tables! The four oldest were previously published and easy to recover, but the most recent, from the 1972 NORC General Social Survey, no longer exists in its original form. I have some hope that it will be possible to recover the counts from the GSS, but surely everyone would have been better served if we had published the counts in the first instance.

I believe it is possible to reduce the printed content of ASA journals, while improving the quantity and quality of scientific communication, by producing a series of machine-readable supplements on floppy diskettes that will include technical appendices, documentation of data, and supporting data, either in raw form (in the case of small bodies of data or survey extracts) or in the form of cross-classification tables and/or moments matrices. I believe that this will improve scientific communication to readers who are primarily interested in reading the journals *per se* and to readers who really want to work through, digest, evaluate, or reanalyze reported research findings. There is little doubt that the installed base of microcomputers is large enough to establish broad access to machine-readable data in the community of active research scholars. I expect that the availability of these types of materials will create a valuable teaching resource.

Obviously, not all papers need be supplemented with machine-readable appendices. It is not clear whether the volume of such material will be great enough to support a supplement for each journal series, or whether a bimonthly or quarterly release might cover all ASA journals appearing during an interval of time. No doubt it is desirable to create standards for documentation to guide editors in recommending material for printed publication and/or for electronic publication. The availability of machine-readable supplements will present authors and editors with serious problems in deciding what material should be printed and what material should be distributed on magnetic media. Initially, machine-readable material will have to be limited to ordinary ASCII files, that is, to the standard 128 characters that all PCs can display and print. The location and scope of responsibility for the accuracy and format of machine-readable submissions will have to be resolved.

The copyright, management, cost, and distribution of machine-readable journal supplements will present significant problems.

It is desirable to consider the distribution of supplementary material in media other than floppy disks. For example, it has been suggested to me that the ASA consider distribution through an electronic bulletin board or a paper (Softstrip) medium for which a \$200 reader must be purchased. The former is used by the American Educational Research Association, and the latter is used to distribute programs by several computer journals.

I believe that all of these issues can be resolved satisfactorily—at least it is worth a try—and that the American Sociological Association ought to take on a leading role in disseminating sociological research through an appropriate mix of printed and machine-readable media. Some appropriate first steps might be to assess the quantity of material in recent volumes that could easily have been produced in machine-readable form in addition to or instead of printed form, and to ask the authors of papers in recent volumes what material they could have released in machine-readable form, if an appropriate outlet had been available. The next step might be an experimental series of releases for one or two major journals.

I made this proposal in an open letter to the chair of the ASA Publication Committee.<sup>1</sup> The reply to my proposal was that “the Committee concluded that current mechanisms for acquiring supplementary material (e.g., by writing to authors of journal articles) were adequate and that no action on the part of ASA was warranted at this time. You are, of course, right that sociologists should share data with greater frequency, but this is a complex problem with no easy solution. As I’m sure you are aware, the Association’s Code of Ethics already mandates data sharing . . . Most members of the Committee felt that requiring article authors to submit supplementary data would not be wise, since it would create more problems than it would solve.”

To be sure, the problem of data sharing will not be an easy one to solve. The fact remains that dissemination of supporting scientific and scholarly materials in machine-readable form is important, and the issue will not go away. Ethical standards for data sharing, such as those of the ASA Code, are meaningless without organizational mechanisms to support them. Indeed, I suggest that the development of such mechanisms by the ASA is mandated by its promulgation of an ethical standard for data

<sup>1</sup> Fortunately, the association has since made a decision to increase the type size in ASA journals, beginning in 1988.

sharing. I hope that other sociologists will join me in encouraging the ASA to undertake an initiative in electronic publishing.

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## CORRECTION

"Estimation with Cross-National Data: Robust and Nonparametric Methods," by Thomas Dietz, R. Scot Frey, and Linda Kalof (ASR 52:380-90).

There are two errors in our paper in the June 1987 issue of ASR on the use of robust and resampling methods to analyze macrosocial indicator data that should be brought to the attention of readers. First, as Professor Kenneth Bollen of the University of North Carolina has pointed out to us, we are incorrect in suggesting that the unbiasedness and efficiency of ordinary least squares (OLS) estimators depend on the normality of the population residual distribution (pp. 381-82). OLS estimates will be unbiased as long as the residuals are independent of the carriers and have an expected value of zero. Further, the OLS estimator will be minimum variance among the class of all linear unbiased estimators if the carriers can be considered non-stochastic, and the residuals are homoscedastic and uncorrelated across observations. If, in addition, the residuals are normally distributed, the OLS estimates will be minimum variance among the class of all unbiased estimators (Hampel et al. 1986, pp. 308-11; Zellner 1983, pp. 129-32). In analyzing macrosocial indicator data, it is sometimes reasonable to assume that some of the observed residuals are sampled from a contaminating distribution with a mean not equal to zero, in which case OLS estimates can be biased. Even if bias is not a problem, in many applications, robust and resampling approaches yield estimates with much smaller mean square error than OLS estimates.

In footnote 11 (p. 388), we discuss options for computing robust and resampling estimators and note the difficulty of conducting such analyses with common statistical packages such

as SAS or SPSS-X. Our pessimism about these packages was overstated. The SAS Supplemental Library (SAS Institute, Inc. 1986) now includes a procedure that jackknifes regression equations, thus providing a resampling estimate of OLS regression coefficients, standard errors, and related statistics. In addition, Professor Nancy Andes of the University of Connecticut has been kind enough to provide us with some SAS macros she has developed with Jack Davis of the Roper Center. These macros will bootstrap any SAS procedure and can draw bootstrap samples that replicate the number of observations in various categories of the data. It appears that the SAS code could easily be reprogrammed in SPSS-X. A listing of these macros can be obtained from her or from us.

We thank Professors Bollen, Andes, and Davis for their most helpful comments.

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## **MANUSCRIPTS FOR THE ASA ROSE SOCIOLOGY SERIES**

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# CROSS-NATIONAL RESEARCH AS AN ANALYTIC STRATEGY\*

## *American Sociological Association, 1987 Presidential Address*

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In this essay, I discuss some of the uses and dilemmas of cross-national research. I argue that cross-national research is valuable, even indispensable, for establishing the generality of findings and the validity of interpretations derived from single-nation studies. In no other way can we be certain that what we believe to be social-structural regularities are not merely particularities, the product of some limited set of historical or cultural or political circumstances. I also argue that cross-national research is equally valuable, perhaps even more valuable, for forcing us to revise our interpretations to take account of cross-national differences and inconsistencies that could never be uncovered in single-nation research.

My thesis is that cross-national research provides an especially useful method for generating, testing, and further developing sociological theory. As with any research strategy, cross-national research comes at a price. It is costly in time and money, it is difficult to do, and it often seems to raise more interpretive problems than it solves. Yet it is potentially invaluable and, in my judgment, grossly underutilized. This is hardly a radically new thesis. As Stein Rokkan (1964) long ago pointed out, to do cross-national research is to return to the preferred analytic strategy of the forefathers of sociology, a strategy that was nearly abandoned in sociology's quest for methodological rigor but now can be pursued anew with the much more powerful methodological tools available today.<sup>1</sup>

A sensible discussion of the uses and dilemmas of cross-national research requires that I first define the domain and delineate the principal types of cross-national research. Then I illustrate some of these uses and dilemmas by scrutinizing the body of cross-national research I know best, namely my own, my rationale being William Form's (1979) cogent observation that "probably no field has generated more methodological advice on a smaller data base with fewer results than has [cross-national] comparative sociology." Using my research as a source of illustrations makes it possible to discuss the issues concretely. I review this research in sufficient detail to highlight its accomplishments and its failures, my concern being only in part with the substance of the research for its own sake; I also want to extrapolate from this concrete example, to make some more general observations. Finally, I discuss some fundamental issues about the conduct of cross-national research. In so doing, I bring in studies dealing with quite different substantive problems from those that I have addressed in my own research, and using quite different methods, to see whether my conclusions apply as well to a much broader range of studies.

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presidents of the American Sociological [Association], among them, William Graham Sumner, W. I. Thomas, E. A. Ross, and Robert E. Park, "exhibited substantial interest in the comparative study of other societies." Between the 1930s and 1950s, these concerns seemed marginal to American sociologists; here they again use ASA presidents as their index, noting that, of the 20 presidents from 1931 to 1950, not one is known primarily or substantially for (cross-national) comparative work. Leaving aside the obvious question of the validity of using the interests of ASA presidents as an index of the substantive concerns of U.S. sociology, I would agree with their generalization and I am intrigued with their explanation. They see the "shift toward parochialism" in U.S. sociology of the 1930s and 40s as resulting from a combination of concern with scientific status, constricting resources, attention to immediate social problems (primarily the Depression and World War II), and the political isolationism of American society during that time. From the vantage point of 1973, Armer and Grimshaw saw a strong revival of cross-national research occurring in the 1960s. So, too, did William Evan (1975), and not only in the United States. In a fascinating analysis, Evan documented the growth of cross-national collaborations and of the "internationalization" of sociology, demonstrating as well the important role of the International Sociological Association in this process.

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I am indebted to my collaborators in cross-national research: Carmi Schooler, Kazimierz M. Slomczynski, Joanne Miller, Carrie Schoenbach, Atsushi Naoi, and (some years ago) Leonard I. Pearlin; to the sponsors of the Polish and Japanese studies: Włodzimierz Wesolowski and Ken'ichi Tominaga; and to colleagues who have critiqued one or another version of this paper: Stephen G. Bunker, Christopher Chase-Dunn, Andrew J. Cherlin, Bernard M. Finifter, William Form, Jonathan Kelley, Janet G. Kohn, Tadeusz Krause, John W. Meyer, Joanne Miller, Jeylan T. Mortimer, Alejandro Portes, Carrie Schoenbach, Carmi Schooler, Theda Skocpol, Kazimierz M. Slomczynski, Katherine Verdery, and Włodzimierz Wesolowski.

<sup>1</sup> Similarly for the United States: Armer and Grimshaw (1973, pp. xi-xii) point out that several of the early

## TYPES OF CROSS-NATIONAL RESEARCH

The broadest possible definition of cross-national research is any research that transcends national boundaries. This definition is somewhat ambiguous, though, because many studies of single societies are implicitly cross-national, in that the investigators interpret their findings by contrasting what they learn about the country they actually study with what is known or is believed to be true about some other country or countries. I prefer to restrict the term, cross-national, to studies that are *explicitly* comparative, that is, studies that utilize systematically comparable data from two or more nations.

In restricting the term to explicitly comparative studies, I do not mean to belittle the importance of studies that are only implicitly comparative. Such studies contribute importantly to our understanding; witness, for example, the distinguished series of studies of American society by foreign observers, beginning with Alexis de Tocqueville's *Democracy in America*. Consider, too, studies in which the selection of some one country is particularly appropriate for testing a general proposition—as in Kelley and Klein's (1981) use of the Bolivian revolution of 1952 to test their theory that "radical revolutions" inevitably lead to an increase in inequality, or Chiro and Ragin's (1975) use of the Romanian peasant rebellions of 1907 to test competing interpretations of the intensity of peasant rebellions. And consider, finally, those pivotal studies—Stephen Bunker's (1985) *Underdeveloping the Amazon* is a particularly good example—where some country or region of a country is selected for study precisely because it exemplifies a more general social phenomenon. I leave such research out of my purview not because it is unimportant, but because to include it would make the bounds of "cross-national" so large and ambiguous that it would be difficult to say what, other than research focused single-mindedly on a particular country, is *not* cross-national.

Within the large genre of research that is explicitly comparative, I would further distinguish four types of cross-national research of somewhat differing intent. The four types are those in which nation is *object* of study; those in which nation is *context* of study; those in which nation is *unit of analysis*; and those that are *transnational* in character.<sup>2</sup> Although these four

types of research shade into one another, their purposes are distinguishable and their theoretical implications somewhat different. My analysis will apply mainly to the second of the four types, in which nation is context of study.

In the first type of cross-national research, where nations are the *object* of study, the investigator's interest is primarily in the particular countries studied: how Germany compares to the United States, France to the Soviet Union, or India to Pakistan. Alternatively, the investigator may be interested in comparing particular institutions in these countries: the social security systems of the U.S. and Australia; the educational systems of the German Democratic Republic and the Federal Republic of Germany. At their best, as in the systematic comparisons of Finland and Poland by Erik Allardt, Włodzimierz Wesolowski, and their collaborators (1978), such studies can lead to well-informed interpretations that apply far beyond the particular countries studied. What distinguishes such research, though, is its primary interest in understanding the particular countries. In this research, one wants to know about Finland and Poland for their own sakes; the investigator does not select them for study just because they happen to be useful settings for pursuing some general hypothesis.

By contrast, I wish to focus on cross-national studies in which, to borrow from Erwin Scheuch's (1967) apt phrase, nation is *context*. In such research, one is primarily interested in testing the generality of findings and interpretations about how certain social institutions operate or about how certain aspects of social structure impinge on personality. In Burawoy and Lukacs' (1987) comparison of a U.S. machine shop with a Hungarian machine shop, for example, their primary interest is not in the United States and Hungary for their own sakes, nor certainly in the particular machine shops, but in these machine shops as exemplifying the relative efficiency of capitalist and socialist industrial enterprises. Admittedly, it may be difficult to differentiate research in which nation is object from research in which nation is context. When Robin Williams (1985) studies the use of threats in US/USSR relations, he

<sup>2</sup> I make no claim that this classification is, theoretically superior to other classifications of cross-national research, only that it serves my analytic purposes better than others do. Compared to Tilly's (1984) well-known classification, my "nation as object" category corresponds roughly to his "individualizing comparisons;" my "nation as context" category encompasses both his "universalizing" and his "variation-finding compari-

sons" (what he sees as two distinct *strategies* of research I see as attempts to interpret two distinct types of findings); my "nation as unit of analysis" category is ignored in his classification; and my "transnational" category may be a little broader than his "encompassing comparisons," which are limited to studies that see nations as components of encompassing international systems. (For other useful classifications of cross-national research, see Hopkins and Wallerstein 1967; Marsh 1967; Elder 1976; and Nowak 1977; see also Hill 1962.)

clearly is interested in the US and the USSR both for their own sakes and as exemplifying superpowers in a nuclear age; there is no way of separating the two purposes. It is nevertheless generally useful to distinguish between research whose primary purpose is to tell us more about the particular countries studied and research whose primary purpose is to use these countries as the vehicle for investigating the contexts in which social institutions operate. My examination of cross-national research as an analytic strategy will be addressed mainly to research where nation is context.

This domain includes such diverse studies as Theda Skocpol's (1979) comparative analysis of revolution, and also, from quite a different theoretical perspective, Michael Burton and John Higley's (1987) analysis of the conditions under which competing elites settle their differences in grand political compromises; Donald Treiman's (1977) analysis of the stratification systems of the industrialized world; William Form's (1976) study of the complexity of industrial technology, workers' skill levels, and the quality of workers' social interactions; Janet Chafetz and Anthony Dworkin's (1986) analysis of the determinants of the size and range of ideologies of women's movements throughout the world; and my colleagues' and my comparative research on social stratification and psychological functioning in Poland, Japan, and the United States (Slomczynski, Miller, and Kohn 1981; Naoi and Schooler 1985).

It is useful to differentiate research where nation is context from two other types of cross-national research that are not central to my discussion here. In the first, where nation is the *unit* of analysis, investigators seek to establish relationships among characteristics of nations *qua* nations. In such research, one no longer speaks of countries by name, but instead classifies countries along one or more dimensions—their gross national product, or average level of educational attainment, or position along some scale of income inequality. A prototypic example is Bornschier and Chase-Dunn's (1985) analysis of the relationship between the penetration of national economies by transnational corporations and the hypothesized long-run stagnation of those economies. Other pertinent examples are Blumberg and Winch's (1972) analysis of the relationship between societal complexity and familial complexity; and Ellis, Lee, and Petersen's (1978) test of the hypothesis that there is a positive relationship between how closely adults are supervised in a society and the degree to which parents in that society value obedience for children.

What distinguishes research that treats nation as the unit of analysis is its primary concern

with understanding how social institutions and processes are systematically related to variations in national characteristics. Such analyses need not treat each nation as a homogeneous entity, but may study intranational institutions and processes, as Meyer, Hannan, and their colleagues (1979) have done in their analyses of national development. Nor need research that treats nation as unit of analysis assume that each nation exists in an international vacuum. As Bornschier and Chase-Dunn (1985, p. 65) put it, "... we do not contend that nation-states are closed systems. A unit of analysis does not need to be a closed system. When we compare individuals or schools we know that these units interact with one another and are parts of a larger social context. The unit of analysis in comparative research is any unit in which the process of interest is known to operate."

In distinguishing research that treats nation as the unit of analysis from research that treats nation as the context for analysis, we are again dealing with gradations, not sharp differences. As will become evident later, attempts to understand cross-national differences sooner or later require one to search for the pertinent dimensions that differentiate the nations *qua* nations. One can, in fact, argue that research in which nation is treated as context is simply a way-station to more general analyses in which the pivotal distinguishing characteristics of nations become variables in the analysis. In principle, as Rokkan (1964), Przeworski and Teune (1970), Hopkins and Wallerstein (1967), and Chase-Dunn (1982) all argue, one can and should convert descriptive differences between countries into analytic variables. I have no quarrel with this objective, only a belief that in many fields of sociological inquiry there is much to learn from research in which nation is treated as context before we are ready to translate "nations" into "variables."

Research that treats nations as the unit of analysis requires that one be able to discern which of the many differences between countries are the pertinent analytic variables; that one be able to formulate meaningful hypotheses at the appropriate level-of-abstraction; and—if one is ever to test such interpretations—that one have at hand or have the potential to collect data from a sizable sample of countries. It also requires much better data than are generally available in multination data sources. I hope that an essay on cross-national research written ten or twenty years from now will be able to focus much more on such research than I believe is warranted today.

And then, finally, there are studies that treat nations as components of larger international systems. Borrowing a term from economists and political scientists who have studied corpora-

tions (and I hope not distorting their usage of the term), I call this *transnational* research. Immanuel Wallerstein's (1974, 1980) analysis of the capitalist world-system and Fernando Cardoso and Enzo Faletto's (1979) analysis of dependency and development in Latin America are prominent examples. We are at a rather early stage in the development of appropriate methodologies for transnational research (Meyer and Hannan 1979; Chase-Dunn 1979; Chase-Dunn, Pallas, and Kentor 1982). Even now, though, transnational research has proved its importance by demonstrating that the nations we compare in *all* types of cross-national research are not isolated entities but are systematically interrelated.

I see all four types of cross-national inquiry as useful, each for particular substantive problems. I focus on research that uses nation as context, not because I consider this type of cross-national research inherently more valuable than the others, but because I think that for many sociological problems—particularly, I must admit, for those in which I have the greatest substantive interest—this type of research has especially great utility in the present state of knowledge. In particular, such research affords the opportunity to study each of the countries with sufficient thoroughness for intensive comparison.

#### ESTABLISHING THE GENERALITY OF RELATIONSHIPS AND THE LIMITS OF GENERALITY

Many discussions of cross-national research (Ragin and Zaret [1983] is a thoughtful example) contrast two research *strategies*—one that looks for statistical regularities, another that searches for cultural or historical differences. I prefer to pose the distinction, not in terms of research strategies, nor of methodological preferences, nor even of theoretical proclivities toward “transhistorical” generalizations or “historically contextualized knowledge,” but in terms of interpreting the two basic types of research *findings*—similarities and differences. Granted, investigators' theoretical and methodological preferences make it more or less likely that they will discover cross-national similarities; granted, too, what can be treated as a similarity at one level of analysis can be thought of as a myriad of differences at more detailed levels of analysis. Still, the critical issue is how to interpret similarities, and how to interpret differences, when you find them.

Finding cross-national similarities greatly extends the scope of sociological knowledge. Moreover, cross-national similarities lend themselves readily to sociological interpretation; cross-national differences are much more diffi-

cult to interpret. As Kazimierz Slomczynski, Joanne Miller, and I argued (albeit a little too categorically) in our first comparative analysis of the United States and Poland:

Insofar as cross-national analyses of social structure and personality yield similar findings in the countries studied, our interpretation can ignore whatever differences there may be in the cultures, political and economic systems, and historical circumstances of the particular countries, to deal instead with social-structural universals. But when the relationships between social structure and personality differ from country to country, then we must look to what is idiosyncratic about the particular countries for our interpretation. (1981, p. 740)

The first half of this formulation asserts that when the relationship between social structure and personality is the *same* in two or more countries, then the unique historical experiences of each country, their distinctive cultures, and their particular political systems are not of focal importance for interpreting the relationship. The formulation does *not* assert that history, culture, and political context have been irrelevant in shaping social structures, but that the resultant social structures have a cross-nationally consistent impact on people. The explanation of this impact should be sought in terms of how people experience the resultant social structures, rather than in the historical or cultural processes that shaped those structures. Admittedly, this may not always be the best interpretive strategy. Apparent similarities can mask profound differences; what seems to call for a unitary interpretation may actually require entirely different explanations. Nevertheless, I believe that where we find cross-national similarities, the most efficient strategy in searching for an explanation is to focus on what is structurally similar in the countries being compared, not on the often divergent historical processes that produced these social-structural similarities. The basic and very simple point is that social-structural similarities may have been brought about by very different historical processes and yet have essentially similar social and psychological consequences.

The second half of the formulation directs us to interpret cross-national *differences* in terms of historical, cultural, political, or economic idiosyncrasies. Przeworski and Teune (1970) argued that what appear to be cross-national differences may really be instances of lawful regularities, if thought of in terms of some larger, more encompassing interpretation. I agree, but I also believe that developing such interpretations is an immensely difficult task. A necessary first step is to try to discover which of the many

differences in history, culture, and political or economic systems that distinguish any two countries are pertinent to explaining the differences we find in their social structures or in how these social structures affect people's lives. I do not contend that cross-national differences cannot be lawfully explained—quite the contrary—but only that the lawful explanation of cross-national differences requires more explicit consideration of historical, cultural, and political-economic particularities than does the lawful explanation of cross-national similarities.

Ultimately, the distinction between cross-national similarities and differences breaks down, and the issues cannot be so simply and neatly dichotomized. Nonetheless, it is a useful way to think about these issues. Therefore, I shall discuss the two types of cross-national research findings separately, beginning with cross-national similarities. I use the U.S.-Polish and U.S.-Japanese comparisons that my collaborators and I have carried out as my principal illustrations of both cross-national similarities and differences, my substantive concern in this part of the essay being the relationship between social structure and personality.<sup>3</sup> The conclusions I draw are by no means limited to this substantive area.

<sup>3</sup> My concern is not with cross-national similarities or differences in *personality* but with cross-national similarities or differences in the *relationship* between social structure and personality. I do not believe that current methods are adequate for assessing whether Poles are more or less intellectually flexible than are Americans, or whether Japanese value self-direction more or less highly than do Americans. Methodological experts whom I greatly respect disagree with this judgment. They believe that if you construct confirmatory factor-analytic models of the same concept for representative samples of two countries, using not only the same indicators of the concept, but also the same reference indicator to establish the metric in both countries, you can compare, e.g., the mean level of authoritarian conservatism for U.S. and Polish adults (Schoenberg 1982). This assumes not only an exact equivalence of meaning, an issue about which confirmatory factor analysis does give us considerable confidence, but also exact equivalence in the frames of reference that people employ in answering questions. I doubt, though, that "strongly disagree" has the same connotations in a Polish interview as in an American interview; the survey specialists of the Polish Academy of Sciences believe that it is difficult for Polish respondents to overcome their cultural tendency to be polite to their guest, the interviewer. We do not have a zero-point for our scales, nor any other basis for mean comparisons. This, however, in no way prevents us from accurately assessing whether, for example, the relationship between social stratification and authoritarian beliefs is of the same sign and of roughly the same magnitude for the United States, Poland, and Japan. And this, I believe, is in any case the more important question for cross-national analysis.

### *Cross-national Similarities*

Over the course of three decades of research in the United States, Carmi Schooler and I, in collaboration with Joanne Miller, Karen A. Miller, Carrie Schoenbach, and Ronald Schoenberg, have intensively studied the psychological impact of social stratification—by which we mean the hierarchical distribution of power, privilege, and prestige (Kohn 1969; Kohn and Schooler 1983). We interpret the consistent relationships that we have found between social stratification and such facets of personality as values, orientations to self and others, and cognitive functioning as the product, in large part, of the intimate relationship between social stratification and particular job conditions. People of higher social-stratification position (as indexed by educational attainment, occupational status, and job income) enjoy greater opportunities to be self-directed in their work—that is, to work at jobs that are substantively complex, free from close supervision, and not highly routinized. The experience of occupational self-direction, in turn, is conducive to valuing self-direction, both for oneself and for one's children, to having self-conceptions and social orientations consonant with such values, and to effective intellectual functioning. It is even conducive to seeking out opportunities for engaging in intellectually active leisure-time pursuits (K. Miller and Kohn 1983). All this is true both for employed men and for employed women (J. Miller, Schooler, Kohn, and K. Miller 1979; Kohn and Schooler 1983; Kohn, Slomczynski, and Schoenbach 1986).

Structural-equation analyses of longitudinal data have enabled us to confirm even that part of the interpretation that posits a causal impact of job conditions on personality (Kohn and Schooler 1978, 1982; Kohn and Schoenbach 1983). These analyses show the relationships to be reciprocal, with job conditions both affecting and being affected by personality. Moreover, analyses of housework (Schooler, Kohn, K. Miller, and K. Miller 1983) and of education (J. Miller, Kohn, and Schooler 1985, 1986) demonstrate that the experience of self-direction, not only in paid employment, but also in housework and schoolwork, decidedly affects people's self-conceptions, social orientations, and cognitive functioning. The interpretation has considerable generality.

In the absence of appropriate cross-national evidence, though, there would be no way of knowing whether this (or any other) interpretation applies outside the particular historical, cultural, and political contexts of the United States. No analyses based solely on U.S. data could tell us whether the relationships between social stratification and personality are an

integral part of the social-stratification system typical of industrial societies, or are to be found only in the United States, or only in countries that have capitalist economies, or only in countries characterized by Western culture, with its purportedly higher valuation of self-direction. Replications of our research by colleagues in other countries (for a review, see Kohn and Schooler 1983, chap. 12), particularly the comprehensive replications that have been carried out by our Polish and Japanese colleagues (Slomczynski et al. 1981; Naoi and Schooler 1985), have made possible tests of the generality of the U.S. findings and of the validity of our interpretation. In the main, these findings are highly consistent with those for the United States, thus greatly enlarging the power of the interpretation.

Of pivotal importance here are the Polish-U.S. comparisons, particularly the comparative analyses of men, for whom the Polish study contains more complete occupational data. The principal issue to which these analyses are addressed is the specificity or generality of the U.S. findings about the linkages of social stratification to job conditions, and of job conditions to personality. Are these linkages specific to the economic and social structures of capitalist society, or do they obtain as well in socialist society?

We have found, for Poland as for the United States, that higher social-stratification position is associated with valuing self-direction, with holding social orientations consonant with such a value—namely, a nonauthoritarian, open-minded orientation, personally responsible standards of morality, and trustfulness (Slomczynski et al. 1981)—and with effective intellectual functioning (Slomczynski and Kohn in press). We have further found a strong reciprocal relationship, for Poland as for the United States, between social-stratification position and occupational self-direction (Slomczynski et al. 1981). Finally, insofar as possible with cross-sectional data, we have shown for Poland, too, a causal impact of occupational self-direction on values, social orientations, and intellectual functioning (Slomczynski et al. 1981; Slomczynski and Kohn in press). Self-direction in one's work leads to valuing self-direction for one's children, to having a more open, flexible orientation to society, and to effective intellectual functioning. Lack of opportunity for self-direction in one's work leads to valuing conformity to external authority for one's children, to viewing social reality as hostile and threatening, and to diminished intellectual flexibility. The effects of social stratification on job conditions, and of job conditions on personality, are much the same in socialist Poland as in the capitalist United States.

This does not mean that these processes are necessarily the same in all socialist and all capitalist societies, but it does mean that the U.S. findings are not restricted to capitalist countries. There is solid evidence, instead, that the interpretive model developed for the United States applies to at least one socialist society.<sup>4</sup>

The United States and Poland, of course, are both Western societies. Are the processes similar in non-Western societies? The Japanese study provides an excellent test of whether our interpretation of the U.S. and Polish findings applies as well to a non-Western industrialized society. In the main, the findings for Japan are markedly consistent with those for the United States and Poland. Social-stratification position is related to values, to social orientations, and to cognitive functioning in the same way, although perhaps not to quite the same degree, as in the United States and Poland (Kohn, Naoi, Schoenbach, Schooler, and Slomczynski 1987). Occupational self-direction has markedly similar effects on psychological functioning in Japan as in the West (Naoi and Schooler 1985). Thus, despite pronounced cultural differences, and despite the sharper division between the primary and secondary sectors of the economy in Japan, the linkages of social stratification to occupational self-direction, and of occupational self-direction to personality, are much the same in Japan as in the United States and Poland. The U.S. and Polish findings are not limited to Western society. Here, again, a single cross-national comparison yields immense benefits for our ability to test the generality of a set of empirical relationships and their interpretation.

Moreover, since the United States, Poland, and Japan are such diverse societies, the set of three studies provides *prima facie* evidence that the psychological impact of social stratification is much the same, and for much the same

<sup>4</sup> The Polish study provides many further examples of cross-national similarity. We have found, for example, that in both Poland and the United States, occupational self-direction not only affects intellectual process, but does so consistently for younger, middle-aged, and older workers (J. Miller, Slomczynski, and Kohn 1985). We have further found that, in both the United States and Poland, the social-stratification position of the parental family has a considerable impact on the values of its adolescent and young-adult offspring (Kohn et al. 1986). The family's stratification position affects both father's and mother's occupational self-direction; each parent's occupational self-direction affects that parent's values; the parents' values affect their children's values. For present purposes, these findings are important primarily because they show how cross-national evidence strengthens the argument that the processes by which social stratification affects values and orientations, even into the next generation, are essentially the same for a socialist and a capitalist society.

reasons, in *all* industrialized societies. Admittedly, negative evidence from research in *any* industrialized society would require a modification of this hypothesis or a restriction of its generality. Admittedly, too, the interpretation speaks only to existing societies. We can say nothing from this evidence as to whether it would be possible to have an industrialized society in which one or another link in the explanatory chain is broken—a society with a less pronounced system of social stratification; a society in which social-stratification position is not so intimately linked with opportunities for occupational self-direction; even a society where occupational self-direction has less impact on personality.<sup>5</sup> Nevertheless, the Polish and Japanese studies do tell us that in decidedly diverse societies—arguably, in all industrialized societies—social stratification is associated with values, social orientations, and cognitive functioning, in large part because people of higher position have greater opportunity to be self-directed in their work.

Whether or not this interpretation is correct, it does illustrate my central point: Where one finds cross-national similarities, then the explanation need not, indeed should not, be focused on the particular histories, cultures, or political or economic circumstances of each of the countries, but instead should focus on social-structural regularities common to them all.

In studying social stratification, I am of course dealing with a feature of social structure that is notably similar in all industrialized societies (Treiman, 1977). I would like to extend the argument a bit, to suggest that even where some feature of social structure is not "identical" in all the countries being compared, but only "equivalent," it is still possible to find cross-nationally consistent relationships between contemporaneous social structure and personality. More than that, it is still appropriate to interpret these consistent relationships in terms of contemporaneous social structure, however much that feature of social structure has been shaped by the particular histories and cultures of those countries.

My illustration here comes from our analysis of position in the class structure and personality in the United States, Japan, and Poland (Kohn et al. 1987). For all three countries, we have adapted the same basic idea—that social classes

are to be distinguished in terms of ownership and control of the means of production, and control over the labor power of others—to the particular historical, cultural, economic, and political circumstances of the country. (For Poland, where ownership of the means of production is not a primary desideratum of class, *control* over the means of production and over the labor power of others is our primary criterion of class position.) The guiding hypothesis is that social class would bear a similar relationship to personality as does social stratification. Hence, we hypothesized that, in all three countries, those who are more advantageously situated in the class structure are more self-directed in their values and orientations, and are more intellectually flexible, than are those who are less advantageously situated. Our further hypothesis, again paralleling what we have learned for social stratification, is that, in all three countries, the explanation lies mainly in the greater opportunities for occupational self-direction enjoyed by those who are more advantaged in class position. The hypotheses, then, are simple extrapolations to social class from what we have consistently found to be the psychological impact of social stratification; the new element is the much greater country-to-country variability of class structures than of stratification systems.

Both hypotheses are confirmed. All three countries can be meaningfully thought to have class structures; class position has similar effects on cognitive functioning, values, and orientation in all three countries; and class affects these facets of psychological functioning for essentially the same reason—because of the intimate relationship between position in the class structure and opportunities afforded for occupational self-direction. Hence, to extrapolate, it is no bar to structural interpretation that social structures have been shaped by distinctly different historical processes.

### *Cross-national Differences*

Interpreting differences, as I said earlier, is where things become much less certain and much more difficult. The key, of course, is the truism that if consistent findings have to be interpreted in terms of what is common to the countries studied, then inconsistent findings have to be interpreted in terms of how the countries—or the studies—differ. This truism, unfortunately, gives no clue as to which of the many differences between countries or between studies lies at the heart of the differences in findings. Prudence dictates that the first hypothesis one entertains is that the inconsistent findings are somehow a methodological artifact. As Bernard Finifter noted:

<sup>5</sup> Michael Burawoy's (1979, p. 13) warning is pertinent, even though our research transcends capitalist society: "By taking the particular experiences of capitalist society and shaping them into universal experiences, sociology becomes incapable of conceiving of a fundamentally different type of society in the future; history is endowed with a teleology whose realization is the present."

There is a curious inconsistency in the way researchers interpret results from attempted replications when discrepancies crop up. Failure to reproduce a finding in the *same* culture usually leads the investigator to question the reliability, validity, and comparability of the research procedures used in the two studies for possible method artifacts. But failure to corroborate the same finding in a *different* culture often leads to claims of having discovered "cultural" differences, and substantive interpretations are promptly devised to account for the apparent differences. (1977, p. 155)

*Issues of method.* The most fundamental methodological issue is whether the concepts employed in the analyses are truly equivalent. Stefan Nowak posed the issue with characteristic clarity:

How do we know we are studying "the same phenomena" in different contexts; how do we know that our observations and conclusions do not actually refer to "quite different things," which we unjustifiably include into the same conceptual categories? Or if they seem to be different, are they really different with respect to the same (qualitatively or quantitatively understood) variable, or is our conclusion about the difference between them scientifically meaningless? (1976, p. 105) (See also Almond and Verba 1963, pp. 57-72; Scheuch 1967, 1968; Smelser 1968; Nowak 1977; Marsh 1967; and Armer 1973).

The issue is so complex that a thorough treatment would require quite another essay. In this essay, instead, I simply assume equivalence of concepts and go on to consider more mundane methodological differences.

In principle, methodological differences between studies could produce either consistent or inconsistent findings (Finifter 1977). Still, when one finds cross-national similarities despite differences in research design, even despite defects in some of the studies, it is unlikely that the similar findings were actually produced by the methodological differences. Substantive similarity in the face of methodological dissimilarity might even argue for the robustness of the findings. But when one finds cross-national differences, then dissimilarities and defects in research design make for an interpretive quagmire—there is no way to be certain whether the apparent cross-national differences are real or artifactual.

It can be terribly perplexing not to know whether an apparent cross-national difference is merely a methodological artifact. I know, for example, of two studies of the interrelationship of social stratification, occupational self-direction, and personality in less than fully

industrialized societies, neither of which shows the pattern that has been consistently found in fully industrialized societies. One study was conducted in Taiwan before that island became as industrialized as it is today (Stephen Olsen 1971), the other in Peru (Scurrah and Montalvo 1975). In Taiwan, the relationship between social stratification and parental valuation of self-direction was essentially the same as has been found in more industrialized societies, but occupational self-direction fails to explain this relationship. In Peru, the correlations of social stratification with such aspects of personality as fatalism, trust, and anxiety are similar to those found in more industrialized societies, but occupational self-direction explains only a modest portion of these correlations.

Should we therefore restrict the interpretation that occupational self-direction is of central importance for explaining the psychological impact of social stratification to apply only to fully industrialized societies? Perhaps we should, and one can readily think of reasons why the interpretation might not apply to partially industrialized societies—for example, the link between social stratification and occupational self-direction may be weaker in such societies. But, since neither the Taiwan nor the Peru study is truly comparable to those done in industrialized societies (see the discussion in Kohn and Schooler 1983, pp. 293-94), the issue is very much in doubt. The Taiwan and Peru studies leave us in a quandary: They raise doubts as to whether the interpretation does apply to partially industrialized societies, but they do not provide convincing evidence that it does not.

To obviate the possibility that differences in findings are merely an artifact of differences in method—in the nature of the samples, in the meaning of the questions asked, in the completeness of data, in measurement—one tries to design the studies to be comparable, to establish both linguistic and conceptual equivalence in questions and in coding answers, and to establish truly equivalent indices of the underlying concepts (Scheuch 1968). Edward Suchman (1964, p. 135) long ago stated the matter with elegant simplicity: "A good design for the collection of comparative data should permit one to assume as much as possible that the differences observed . . . cannot be attributed to the differences in the method being used." Unfortunately, one can never be certain. The best that is possible is to try to establish damage control, to present whatever evidence one can that methodological incomparables are not so great as to explain the differences in findings. Short of that, it remains a gnawing doubt.

My colleagues and I have written extensively about the technical issues in achieving true cross-national comparability, particularly those



involved in interviewing and in index construction (J. Miller, Slomczynski, and Schoenberg 1981; Slomczynski et al. 1981; J. Miller et al. 1985; Kohn et al. 1986). So, too, have many other scholars (see, in particular, Scheuch 1968; Przeworski and Teune 1970; Armer 1973; Elder 1976; Kuechler 1986). Therefore, I do not discuss these issues further here. Instead, I assume comparability of methods (as well as comparability of concepts) and go on to the equally perplexing substantive issues in interpreting cross-national differences.

*Substantive interpretations of cross-national differences.* Finding a cross-national difference often requires that we curtail the scope of an interpretation, by limiting our generalizations to *exclude* implicated variables or relationships or types of countries from a more encompassing generalization. Ultimately, though, we want to *include* the discrepant findings in a more comprehensive interpretation by reformulating the interpretation on a more general level that accounts for both similarities and differences. Thus, although the discovery of cross-national differences may initially require that we make a less sweeping interpretation, in time and with thought, it can lead to more general and more powerful interpretations.

I wish that I could offer from my research an example of a powerful reinterpretation derived from coming to terms with cross-national differences. Instead, I can only share with you my dilemma in still not fully understanding some differences that I have been struggling to understand for some years. I may not convince you that discovering cross-national differences necessarily leads to new understanding, but I shall certainly convince you that the discovery of such differences forces one to question generalizations made on the basis of studying only one country. To illustrate, I use the most perplexing cross-national inconsistencies that we have found in the U.S.-Polish-Japanese comparisons (Kohn et al. 1987).

Quite in contrast to our consistent findings about the relationship of social stratification to other facets of personality, we have found a decided inconsistency in the relationship between social stratification and a principal underlying dimension of orientations to self and others—a sense of well being *versus* distress. In the United States, higher stratification position *decreases* feelings of distress; in Japan, there is virtually *no* relationship between social stratification and feelings of distress; and in Poland, higher stratification position *increases* feelings of distress.<sup>6</sup> The

magnitude of the correlation is not great in any country, but the inconsistency in direction of relationship is striking. Similarly for social class: In the United States, members of more advantaged social classes, managers in particular, have a greater sense of well-being; members of less advantaged social classes, blue-collar workers in particular, have a greater sense of distress. In Poland, quite the opposite: It is the managers who are more distressed, the blue-collar workers who have a greater sense of well-being. In Japan, as in the United States, managers have a strong sense of well-being, but it is the white-collar—not the blue-collar—workers who are most distressed.

Why don't advantageous positions in the stratification and class systems have cross-nationally consistent effects on the sense of distress? On one level, this question is readily answered: Our analyses show that stratification and class matter for psychological functioning primarily because people of more advantaged position have greater opportunity to be self-directed in their work. But we find, in causal models of the reciprocal effects of occupational self-direction and distress, that although occupa-

aspects of social orientation, but affects *some* aspects of self-conception differently. In particular, in the United States, higher stratification position is associated with greater self-confidence and less anxiety; in Poland, quite the opposite.

"Social orientation" and "self-conception," however, are merely convenient rubrics; they are not underlying *dimensions* of orientation. Schooler and I (Kohn and Schooler 1982; 1983, Chapter 6) subsequently did a second-order confirmatory factor analysis of the several first-order dimensions of orientation, using U.S. data, to demonstrate that there are two underlying dimensions: self-directedness of orientation *versus* conformity to external authority, and a sense of well-being *versus* a sense of distress. Self-directedness of orientation implies the beliefs that one has the personal capacity to take responsibility for one's actions and that society is so constituted as to make self-direction possible. It is reflected in not having authoritarian conservative beliefs, in having personally responsible standards of morality, in being trustful of others, in not being self-deprecatory, in not being conformist in one's ideas, and in not being fatalistic. Distress is reflected in anxiety, self-deprecation, lack of self-confidence, nonconformity in one's ideas, and distrust. We have since shown that these same two dimensions underlie the several facets of orientation in Poland and in Japan (Kohn et al. 1987). The basic parameters of the Polish and Japanese models, in particular the relationships between second-order and first-order factors, are quite similar to those for the U.S. model. In all three countries, there is a strong positive relationship between social stratification and self-directedness of orientation. The relationship between social stratification and the sense of distress, however, is neither strong nor cross-nationally consistent: the correlations are  $-0.18$  for the United States,  $-0.01$  for Japan, and  $+0.15$  for Poland.

<sup>6</sup> In our original comparative analysis of the United States and Poland (Slomczynski et al. 1981), we put the issue somewhat differently: Social stratification has similar effects in the United States and Poland on all

tional self-direction has a statistically significant effect (negative, of course) on the sense of distress for the United States and Japan, it has no effect at all for Poland. This is in marked contrast to the cross-nationally consistent effects of occupational self-direction on intellectual flexibility, values, and self-directedness of orientation. One can, in fact, incorporate the cross-national inconsistency into an encompassing generalization: Where occupational self-direction has cross-nationally consistent effects on psychological functioning, so too do social stratification and social class; where occupational self-direction fails to have consistent effects, stratification and class also have inconsistent effects.

On another level, though, the question persists: *Why* doesn't occupational self-direction mitigate against distress in Poland, as it does in the United States and Japan? Moreover, occupational self-direction does not provide as effective an explanation of the relationships of stratification and class with distress in *any* of the three countries as it does for their relationships with other facets of personality in *all* three countries. Given the rather substantial effect of occupational self-direction on distress for the United States, we might well expect a higher correlation of social stratification with distress than the  $-0.18$  that we actually do find. We should *certainly* expect a higher correlation than the  $-0.01$  that we actually do find for Japan. We should expect *no* relationship, not a positive relationship, for Poland. Clearly, more than occupational self-direction is involved in explaining the relationships of stratification and class to distress. My formulation, which implies that occupational self-direction, and therefore also stratification and class, would have an impact on feelings of distress consistent with its impact on values, self-directedness of orientation, and cognitive functioning, must be revised.

It is not at all certain from the evidence at hand, though, whether the interpretation requires minor revision or extensive overhaul. I am reasonably certain that the cross-national differences are not merely a methodological artifact, for example in the conceptualization or measurement of distress. In particular, the cross-national differences are found, not only in analyses using the "higher-order" concept, distress, but also in analyses using the "first-order" concepts, notably self-confidence and anxiety (see note 6). The issues are substantive, not methodological.

In any reformulation, it is essential that we not lose sight of the fundamental principle that any explanation of cross-national *differences* must also be consistent with the cross-national *similarities*. To be valid, any explanation has to explain why we find cross-national inconsisten-

cies *only* for the sense of distress, not for values, for self-directedness of orientation, or for cognitive functioning. Explanations so broadly framed as to lead one to expect Polish or Japanese men of more advantaged position to value conformity for their children, to have a conformist orientation to self and society, or not to be intellectually flexible, could not be valid. Nor would it make any sense to explain the findings in terms of a weaker linkage of social stratification or of social class to occupational self-direction in Poland or in Japan than in the United States, or in terms of occupational self-direction being any less important for Polish or Japanese men than for U.S. men.

As I see it, there are at least five ways that my interpretation might be reformulated:

The simplest reformulation would be to limit the scope of the interpretation to exclude the sense of distress; for as-yet unknown reasons, an interpretation that does apply to cognitive functioning, values, and self-directedness of orientation seems not to apply to the affective realm. This reformulation simply curtails the scope of my interpretation, until such time as we are able to develop a more general interpretation that incorporates cross-national differences along with cross-national similarities.

A second type of reformulation would posit that the psychological mechanisms by which job conditions affect distress may be different from those by which job conditions affect cognitive functioning, values, and self-directedness of orientation. Such a reformulation might or might not emphasize job conditions different from those that I have emphasized; it certainly would posit different *processes* by which job conditions affect personality. Mine is a learning-generalization model: People learn from their job experiences and apply those lessons to non-occupational realms of life (Kohn 1985). One could argue that the inconsistent effects of occupational self-direction on the sense of distress raise questions as to whether a learning-generalization model applies to this facet of personality. Perhaps, instead, one should employ some other model of psychological process—a "stress" model is the obvious candidate—for understanding the effects of job on the sense of distress. The "stress" model posits that job conditions affect personality, in whole or in part, because they induce feelings of stress, which in turn have longer-term, off-the-job psychological consequences, such as anxiety and distress. Clearly, "stress" is a plausible link from job conditions to distress. But I think the evidence for a "stress" model, even when applied only to anxiety and distress, is less than compelling (Kohn 1985); moreover, positing different mechanisms for different facets of personality would be, at best, inelegant.

A related possibility, one that is much more to my liking, retains the learning-generalization model but expands the range of pertinent job conditions. This reformulation begins with the U.S. finding that job conditions other than those directly involved in occupational self-direction are more important for distress than for other facets of personality (Kohn and Schooler 1982; 1983, Chapter 6). *Some* of these job conditions are related to stratification and class, hence might explain the effects—or lack of effects—of stratification and class on distress. The crux of this reformulation is the hypothesis that the effects of these other job conditions on distress may be at odds with, and perhaps more important than, those of occupational self-direction. We have some pertinent, albeit limited, evidence that lends credence to this possibility (Kohn et al. 1987). In the United States, for example, job protections (such as seniority provisions in union contracts) mitigate against distress. Nonetheless, the very people who at the time of our interviews enjoyed the greatest job protections—the blue-collar workers—were also the most distressed. Blue-collar workers were distressed *because* they lacked opportunities for occupational self-direction and *despite* the job protections that many of them, particularly union members, enjoyed. Occupational self-direction and job protections seem to have countervailing effects, which may account for the relatively modest relationships of both social stratification and social class with distress, even in the United States.

For Japan, we find that believing that one works under considerable pressure of time, and believing that people in one's occupation are at risk of being held responsible for things outside of their control, are both related to distress. Although these findings may merely reflect a propensity of distressed people to overestimate the pressures and uncertainties of their jobs, it is at least a plausible hypothesis that such job conditions do increase distress. Our causal models suggest as well that either education itself, or job conditions related to education, increases distress. The countervailing effects of occupational self-direction, education, and other job conditions correlated with them both, may help explain why stratification and class have so little net effect on distress in Japan.

For Poland, we lack information about job conditions other than those directly pertinent to occupational self-direction. We do, however, have one fascinating bit of information that may help explain what it is about the conditions of life experienced by Polish *managers* that makes them more distressed than members of other social classes, quite in contrast to the situation of managers in the United States and Japan. We find that one segment of the Polish managerial

class is particularly distressed—those managers who are not members of the Polish United Workers (Communist) Party. There are too few non-Party managers for this finding to be definitive, but I think it suggestive that the non-Party managers have decidedly higher levels of distress, compared not only to managers who are members of the Party, but also compared to members of any other social class, Party members or not. The implication, I think, is that being a non-Party manager in the Polish system of centralized planning entails uncertainties, risks, and insecurities greater than those experienced by managers who are members of the Party, and greater than those experienced by managers in the less centralized systems of capitalist countries. The Polish system may hold these managers responsible for accomplishments they have neither the leeway nor the resources to achieve. By the same token, the U.S. and Japanese systems may lead managers to feel more in control of the conditions of their lives than they really are.

Our evidence suggests, then, that not only does occupational self-direction fail to have the cross-nationally consistent effect on distress that it has on other facets of psychological functioning, but also, that other job conditions associated with stratification and class may have countervailing effects. What is lacking is adequate information about these other job conditions.

A fourth type of reformulation would take greater account of the processes by which people attain their occupational positions and of the meaning these positions have to them. Slomczynski, Miller, and Kohn (1981) speculated at length about the implications of post-World War II historical developments that resulted in differences between the United States and Poland in structural mobility, job-selection processes, and the symbolic importance attached to class position—differences that might explain why social stratification bears a different relationship to distress in the two countries. These speculations still seem to me to be plausible and they are certainly potentially testable. One could similarly point to differences between Japan and the West in the structure of industry, particularly in the sharper division in Japan between primary and secondary sectors of the economy, that might be pertinent to explaining why stratification has so little relationship to distress in Japan, and why Japanese white-collar workers are more distressed than are members of other social classes.

Finally, one could broaden the scope of the interpretation even more, by taking account of conditions of life other than those involved in job and career. It might be, for example, that cross-national differences in family structure, or

in religious belief, or in whether the urban population is primarily rural in origin, or in "national culture" bear on the sense of distress. The pivotal questions, though, are not whether family, religion, rural origins, or culture account for differences in Polish, Japanese, and American men's sense of distress, but whether such non-occupational conditions help explain why *social stratification* and *social class* bear different relationships to the sense of distress in Poland, Japan, and the United States.

We do not have the evidence to test any of these interpretations. Each type of reformulation (other than simply limiting the scope of the interpretation to exclude distress) would require a different type of data. To test a "stress" formulation would require more information about the relationship between objective job conditions and the subjective sense of "stress" in one's work, and about the relationship between job *stress* and off-the-job *distress*. Similarly, to test any other model of psychological process would require data directly pertinent to that formulation. To test the hypothesis that job conditions other than those involved in occupational self-direction help explain the relationships of social stratification and social class to distress would require that we obtain much fuller information in all three countries about those job conditions thought to be productive of a sense of distress. To test the hypothesis that different processes of educational and occupational attainment account for the differential effects of stratification and class on the sense of distress would require information of yet another type: historical information about the impact of changes in the educational and occupational structures of Poland, Japan, and the United States since World War II as they impinged on particular cohorts of Polish, Japanese, and American workers. And then, finally, to test the rather vaguely formulated hypothesis that non-job conditions explain the cross-nationally inconsistent relationships of both class and stratification with distress would require information about the interrelationship of stratification and class with these other lines of social and cultural demarcation, in all three countries.

In any case, on the basis of presently available evidence, I still do not have a fully adequate explanation of why social stratification and social class have cross-nationally inconsistent effects on the sense of distress. Perplexed though I am, I value the cross-national evidence for making clear where my interpretation applies and where it does not, thus defining what is at issue. Were it not for the Polish and Japanese findings, there would have been little reason to doubt that my interpretation applies, albeit not quite as well, to the sense of distress, just as it

clearly does to values, self-directedness of orientation, and cognitive functioning.

### SOME GENERAL CONSIDERATIONS

I can now address some more general issues about cross-national research that I deliberately deferred until I had offered some concrete examples. These remarks are primarily addressed to research in which nation is treated as context.

1. *In whose interest is cross-national research?* This seemingly innocuous question contains a range of serious ethical and professional issues. At its worst, as in the infamous Camelot affair (Horowitz 1967), cross-national research has been used in the service of political oppression. In a less dramatic way, cross-national research has too often been a mechanism by which scholars from affluent countries have employed scholars in less affluent countries as data-gatherers, to secure information to be processed, analyzed, and published elsewhere, with little benefit either in training or in professional recognition for those who collected the data (Portes 1975; Scheuch 1967). These are complex issues, where surface appearances may be misleading. But, certainly, the history of cross-national research has not been entirely benign.

Past sins and mistakes notwithstanding, cross-national research need not be employed in the service of academic or other imperialisms. My own research is again illustrative. As a matter of historical record, it was not I but Włodzimierz Wesolowski (1975, p. 98) who proposed the Polish-U.S. comparative study. He did so for precisely the same reason I found the prospect so attractive when he suggested it to me: to see whether the U.S. findings would apply to a socialist society. The study was funded and carried out by the Polish Academy of Sciences, who thought the issues important for Polish sociology and Polish society. The extension of the U.S.-Polish comparison to encompass Japan came about because Ken'ichi Tominaga, his Japanese colleagues, and the Japanese universities and foundations that funded this research were as interested as were the Americans and the Poles in seeing whether these phenomena are similar in that non-Western society.

The opportunities for genuine cross-national collaboration today, when there is a thriving, highly professional sociology in many parts of the world, are much greater than they were only a few years ago. Today it is quite possible, and advantageous for all concerned, for sociologists of many countries to collaborate effectively. The theoretical and policy issues to be addressed in cross-national research can be—in principle,

ought to be—equally important for sociologists of all the countries concerned.

2. *Is cross-national research distinctly different from research that compares social classes, or ethnic groups, or genders in a single country?* I see cross-national research as one type of comparative research. In many discussions, though (see, for example, Armer and Grimshaw 1973), the term "comparative research" is treated as synonymous with cross-national research, as if the only possible comparison were *inter-national* comparison; this I regard as hubris on the part of the internationalists. In other discussions (e.g., Hopkins and Wallerstein 1967) the term "comparative" is used more broadly and "cross-national" is limited to what I consider to be only one type of cross-national research, transnational research. And in still other discussions (e.g., Ragin 1982), comparative research is seen as that particular type of cross-national research where "society" is used as the explanatory unit.<sup>7</sup> These varying usages seem to me to impede meaningful discourse. I think it best to use the commonsense meanings of both "comparative" and "cross-national."

My own research shows that cross-national research is no different in principle from other comparative research, although in practice it is likely to be more complex, especially as one tries to interpret cross-national inconsistencies. What makes it worth distinguishing cross-national research from other types of comparative research is that a much broader range of comparisons can be made: comparisons of political and economic systems, of cultures, and of social structures. Any comparisons we make within a single country are necessarily limited to the one set of political, economic, cultural, and historical contexts represented by that particular country. I simply cannot imagine any study of the psychological impact of class and stratification, done entirely within the United States, that could have extended the scope of our knowledge, or the power of our interpretation, as greatly as did the Polish and Japanese studies.

3. *Why put the emphasis on cross-national? Why not cross-cultural or cross-societal or cross-systemic? Doesn't the term cross-national ascribe a greater importance to the nation-state than it deserves?* I use the term *cross-national* mainly because *nation* has a relatively unambiguous meaning. *Cross-cultural* can mean anything from comparing subcultures within a

single nation, for example, comparing Mexican-American and Anglo-American subcultures in the Southwest region of the United States, to comparing very large groupings of nations that share broadly similar cultures, as in William Goode's (1963) comparative analyses of historical changes in family patterns in "the West," Arabic Islam, Sub-Saharan Africa, India, China, and Japan. Similarly, as Charles Tilly (1984) cogently argues, it is extremely difficult to define what is a "society." And the term *cross-systemic* is so vague as to have little research utility.

I do not think that this usage of *nation* necessarily implies anything about the importance of nation, or the nation-state, as such, any more than cross-cultural implies (or, at any rate, should imply) that *culture* is the explanatory desideratum. Furthermore, we learn something about the importance or lack of importance of the nation-state by discovering which processes transcend national boundaries and which processes are idiosyncratic to particular nations or to particular types of nations. In choosing which nations to compare, sometimes we do mean to compare nation-states; how could Theda Skocpol (1979) have done differently in her analyses of revolutions? When we deal with governments, laws, and legally regulated institutions, the nation-state is necessarily a decisive context. But sometimes we use *nation* as a way of comparing cultures; in this case, we would choose nations with distinctly different cultures, for example, by comparing the United States to Japan, not the Federal Republic of Germany to Austria. Sometimes we mean to compare political and economic systems, as in comparing the United States and Japan to Poland, or if one wanted to minimize cultural differences while contrasting political systems, in comparing the German Democratic Republic to the Federal Republic of Germany. Cross-national research is flexible, offering the advantage of making possible multiple types of comparison within one general analytic framework.

This flexibility, it must be recognized, comes at a price: When one finds cross-national differences, it may not be clear whether the crucial "context" that accounts for the differences is nation or culture or political or economic system (Scheuch 1967). Still, one can at least try to assess which of these contexts might logically be pertinent to explaining a particular cross-national difference. And, for many types of research, one can then proceed to design new studies to differentiate among the contexts.

4. *How many nations are needed for rigorous cross-national analysis, and how should they be chosen?* For some purposes, particularly when using secondary data to establish cross-national

<sup>7</sup> The issues in distinguishing cross-national research from other comparative research are discussed thoughtfully and at length by Grimshaw (1973), who, *inter alia*, reviews and summarizes pertinent earlier discussions by Erwin Scheuch and Neil Smelser. See also Marsh (1967) and Zelditch (1971).

generalities, it is desirable to include all countries for which pertinent data can be secured. Thus, Alex Inkeles's pioneering paper, "Industrial Man," (1960) gained considerably from its demonstration that the relationship between social stratification and many facets of values and beliefs is consistent for a wide array of countries. Seymour Martin Lipset's argument in "Democracy and Working Class Authoritarianism" (1959), that the working class is more "liberal" than the middle class on economic issues, but illiberal on issues of civil liberties and civil rights, was the more forceful because he marshalled evidence from several countries. Donald Treiman's (1977) comprehensive analysis of the similarity of social stratification systems throughout the industrialized world effectively utilized data from many countries and was enriched as well by information about the historical past. Janet Chafetz and Anthony Dworkin's (1986) analysis of the size and range of ideologies of women's movements gained scope and power from their use of data from a considerable diversity of countries. With similar intent, I have searched for all extant studies to establish the "universality" of a self-direction/conformity dimension to parental values in industrialized societies (Kohn and Schoenbach 1980). I have also searched for evidence in studies conducted in many countries for cross-national tests of one or another link in my explanatory schema (Kohn 1977, 1981; Kohn and Schooler 1983, Chapter 12). And, as recently as the July 1987 issue of the *American Journal of Sociology*, Alejandro Portes and Saskia Sassen-Koob demonstrated anew the usefulness of a broad comparative sweep, in showing that, contrary to all theoretical belief, the "informal," "underground" sector of the economy is not merely a transitional phenomenon of Third World development, but is instead a persistent and integral part of the economies of even advanced capitalist nations. In doing secondary analyses it is highly advantageous to utilize data from all countries for which pertinent information can be secured.

Moreover, even in collecting primary data, there can be considerable advantage to assessing the consistency of findings across a range of nations, cultures, and political systems, as Inkeles and Smith showed in *Becoming Modern* (1974) and as Erik Olin Wright and his colleagues are demonstrating anew, in a very different type of research endeavor, in their multi-nation studies of social class.

Yet, it is expensive, difficult, and time-consuming to collect data in many countries. We are rarely able to collect reliable data about enough nations for rigorous statistical analysis. Nor are we ordinarily able to study many countries in sufficient depth for intensive

comparison. It is not necessarily true that the more nations included in the analysis, the more we learn. There is usually a tradeoff between number of countries studied and amount of information obtained. In this tradeoff, investigators can certainly disagree about the relative importance of number of countries and depth of information. And the same investigator might make different choices for different substantive problems. By and large, though, I would opt for fewer countries, more information.

My own preferred strategy is the deliberate choice of a small number of nations that provide maximum leverage for testing theoretical issues. One may begin with a study in one country, with subsequent extensions of the inquiry to other countries, as my collaborators and I have done in using Poland to learn whether U.S. findings are applicable to a socialist society and Japan to learn whether such findings apply to a non-Western, industrialized society. Alternatively, one can select pivotal countries that provide maximum opportunity to test some general hypothesis, as Theda Skocpol (1979) did in selecting France, Russia, and China for her study of the causes and consequences of social revolutions, or as John Walton (1984) did in selecting the Philippines, Colombia, and Kenya for his comparative analysis of national revolts in underdeveloped societies. Whether one starts with one country and then extends the inquiry to others, or begins with a small set of countries, does not seem to be crucial. Either way, the deliberate choice of a small number of countries for *systematic, intensive study* offers maximum leverage for testing general propositions about social process.

How, then, does one decide which countries to compare? The only rule of thumb I know is that cross-national research is most useful when it can resolve a disputed question of interpretation. It follows that what is a strategic comparison at one stage of knowledge may be overly cautious or overly audacious at another.

At an early stage of my own research, for example, when I had established little more than that white middle-class parents in Washington, DC valued self-direction for their children more highly than did white working-class parents in that same city at that one time, the focal issue was Washington's atypicality. Was the Washington finding peculiar to the times and circumstances of this relatively affluent, economically secure, mainly non-industrial city in the late 1950s, or did that finding reflect a more general relationship between social stratification and parental values? Leonard Pearlman (1971; Pearlman and Kohn 1966) resolved this question by demonstrating a similar relationship of social stratification to parental values in Turin, Italy—an industrial city, less affluent and less

economically secure than Washington, and with a much less conservative working-class tradition. A more cautious choice of locale would have been an industrial city in the United States or perhaps in English-speaking Canada or in Australia. A more audacious choice would have been an industrial city in a non-Western country or in a socialist country. Turin, to my mind, was neither too cautious nor too audacious a choice: different enough from Washington that if the findings proved to be similar, the increment to our knowledge would be considerable, but not so different from Washington that if the findings had proved to be dissimilar, we would have been at a complete loss to know why. Turin was not the only city that could have served our purposes; several other West European cities might have served as well. In that state of our knowledge, though, I do not think that Warsaw or Tokyo would have been optimal choices. It would have been too difficult to interpret dissimilar findings.

Later, when we had solid evidence about the generality of our findings in Western, capitalist societies, studies in Poland and Japan became especially useful. The issue was no longer Washington's atypicality, but whether the relationships among social stratification, job conditions, and psychological functioning were peculiar to capitalist society or to Western society. Here, again, we could have chosen other countries that might have served our purposes as well: perhaps Hungary instead of Poland, or if it had been possible to do such research there at that time, the Soviet Union; perhaps South Korea instead of Japan. It is often the case that no one country is uniquely appropriate for cross-national comparison. Other considerations—research feasibility, the availability of potential collaborators, funding, happenstance—may then legitimately enter in.

Were I to embark on a new comparative study today, the considerations would again be different, mainly because of what we now know from the Polish and Japanese studies, and because of new interpretive problems that have arisen from these studies. It would now be useful to study another socialist country and another non-Western industrialized country. It would also be useful to study a less than fully industrialized country, I think preferably (for the nonce) a capitalist country with a predominantly Western culture, perhaps a Latin American country. The possibilities for fruitful comparison do not shrink as one learns more, but actually grow.

The choice of countries should always be determined by asking whether comparing these particular countries will shed enough light on important theoretical issues to be worth the investment of time and resources that cross-

national research will certainly require (Galtung 1967, p. 440). One must always ask: If I find cross-national consistencies, will this particular cross-national comparison extend the scope of my interpretation enough to have made the venture worthwhile? And if I find differences, will this particular cross-national comparison shed light on crucial interpretive problems? Cross-national research is always a gamble; one might as well gamble where the payoff is commensurate with the risk.<sup>8</sup>

5. *What are the costs of doing cross-national research?* If, as I have argued throughout this essay, the advantages of cross-national research are considerable, so too are the costs. These costs are considerably greater than most investigators realize, great enough to make a rational person think twice about doing cross-national research when it is not needed or when it is premature.

Securing funds is always problematic, even (as in my own research) when financial support is obtained in the countries that are participating in the research. This, however, is only the first and by no means the most serious difficulty. Establishing collaborative relationships that can be sustained and will develop throughout the course of what can be counted on to be difficult research is much more problematic (Hill 1962; Sarapata 1985). Both the greatest benefits and the most difficult problems of cross-national research come from the collaborative relationships. If a good collaboration is like a good marriage, rewarding yet difficult, then a good cross-national collaboration is akin to a cross-cultural marriage that manages to succeed despite the spouses living much of the time in different countries, sometimes with considerable uncertainty about passports, visas, and the reliability and timeliness of mail delivery, and despite working in different institutional settings with conflicting demands and rewards. And still, it's far preferable to the alternatives. More than that, without good collaboration, many types of cross-national research are simply not possible.

The methodological pitfalls are another set of obstacles to good cross-national research; I have touched on some of them earlier in this essay. It would be hard to exaggerate the amount of time, thought, and analysis that must go into the effort

<sup>8</sup> A corollary is that, if one wants to gamble audaciously, do so where the payoff will be considerable. A splendid example is provided by Nancy Olsen (1974). She not only extended to Taiwan the scope of our U.S. findings about the relationship between closeness of supervision and parents' values for their children, but also extended the scope of generalization about the institution in which close supervision is experienced, from paid employment to the family itself.

to achieve comparability of methods, concepts, and indices. There are also issues in the standards of research employed in different countries. Sometimes these issues become acutely problematic when one least expects them. As a simple yet telling example: The reason why we do not have Polish data about some of the job conditions that may be pertinent to distress is that the survey research specialists at the Polish Academy of Sciences refused to include questions about job conditions that did not meet their criteria of objectivity in a survey for which they were professionally responsible. Even when we appealed to them that cross-national comparability required their repeating the defects of the earlier U.S. study, they would not yield. They were as zealous in imposing their justifiable, yet irrelevant professional standards as were the clearance officers of the U.S. Department of Health, Education, and Welfare, and of the Office of Management and the Budget, in imposing their not nearly so justifiable requirements.

And still, there are yet more difficult problems, problems of interpretation. Particularly when one finds cross-national differences, an expert knowledge of all the countries is essential—a knowledge most easily achieved, of course, by collaborators who have expert knowledge of their own countries (see Kuechler 1986). Even when such collaboration exists, though, sharing knowledge, interpreting within a common framework, even having enough time together to think things through at the crucial junctures, does not come easily.

Unless one has a good reason why research *should* be cross-national, it generally isn't worth the effort of making it cross-national. Operationally, this means that one should do cross-national research either when a phenomenon cannot be studied in just one country (for example, the causes of revolutions) or else when some phenomenon has been well substantiated in one country and the next logical questions have to do with the limits of generality of what has been learned. In principle, but rarely in practice, it may be worth embarking on a cross-national study of a less well researched problem if you have good *a priori* reason to believe that important theoretical issues can be more effectively addressed by conducting the research in more than one country. I remain a strong proponent of cross-national research, but I would not wish to mislead anyone into thinking that its very considerable advantages do not come at equally considerable cost.

6. Finally, to return to a question that has pervaded this essay: What role does history play in cross-national interpretation? In posing this question, I most decidedly do not mean to cast doubt on the utility of historical analysis as a

method for doing cross-national research. I regard the persistent debate about the relative merits of historical and quantitative methods in cross-national research as a wasteful distraction, addressed to a false dichotomy.<sup>9</sup> Each method is appropriate for some research purposes and inappropriate for others. Best of all, as Jeffery Paige (1975) demonstrated in his analysis of the relationship between agricultural organization and social movements in 70 developing nations, is to combine the two. My question concerns, not historical analysis as method, but history as explanation. At issue, of course, are the competing merits of idiographic and nomothetic explanation. I can hardly do justice to this complex question in the closing paragraphs of this essay, but I would at least like to point out that the issues are somewhat different when analyzing cross-national similarities from what they are when analyzing cross-national differences.

As I have argued throughout this essay, the interpretation of cross-national *similarities* should not focus on the unique historical experiences of each of the countries. One seeks to discover, instead, social-structural regularities that transcend the many differences in history, culture, and experience that occur among nations. This is true even in inquiries—Walton's (1984) *Reluctant Rebels* is a good example—where the evidence is mainly historical but the analysis searches, not for historical idiosyncrasies, but for historical commonalities. The intent in all analyses of cross-national similarities is to develop generalizations that transcend particular historical experiences in a search for more general explanatory principles. In short, the method may be historical, the interpretation should be sociological.

In a broader sense of history, of course, cross-national analysis, just as any other type of sociological analysis, cannot be ahistoric, even when much about history is only implicit in the interpretation (Sztompka 1986). To compare the impact of social stratification on personality in the United States and Poland, for example, assumes that we are comparing industrialized states that have shared much of Western history.

<sup>9</sup> The methodological debate takes place on two levels: the type of analysis used *within* each nation and the type of analysis used for *comparing* nations. I see nothing of value in the first part of the debate; one uses whatever methods are appropriate to the task. The second part of the debate deals with real issues, for example, the meaningfulness of using "samples" of nations, the utility of statistical tests when basing one's analysis on the entire set of existing countries, and the difficulties of having to test multiple interactions on a necessarily small number of "cases" (see, e.g., Ragin 1982). This literature, despite its antiquantitative bias, offers some useful cautions.



That one is a capitalist state and the other a socialist state can be viewed, depending on how you read the broad sweep of history, as a comparison of different economic-political systems or as a comparison of different levels of political development. In either case, even though history is not treated explicitly, historical considerations are certainly there implicitly. And when one compares fully industrialized to partially industrialized societies, historical issues are necessarily at least implicit. Nevertheless, in interpreting cross-national similarities, history need not be at the forefront of attention.

In interpreting cross-national differences, by contrast, historical considerations cannot be merely implicit; history must come to the forefront of any interpretation. For example, after demonstrating remarkable parallels in both the causes and consequences of the French, Russian, and Chinese revolutions, Skocpol (1979) had to explain differences, particularly in revolutionary outcomes, in terms of historically unique circumstances. Similarly, when I find that social stratification and social class do not have the same impact on the sense of distress in the United States, Poland, and Japan, I have to look to the separate historical developments of the three countries, to try to discover what may explain the inconsistent findings. I maintain, though, that even in interpreting cross-national differences, explanation cannot consist merely in explicating pertinent historical differences. The object is not an understanding of history just for history's sake, but the use of history for understanding more general social processes. The interpretation must be historically informed, but sociological interpretations, even of cross-national differences, are quintessentially trans-historical.

## EPILOGUE

In the preface to *Class and Conformity*, I made a declaration of faith: "The substance of social science knowledge comes from the process of speculation, testing, new speculation, new testing—the continuing process of using data to test ideas, developing new ideas from the data, doing new studies to test those ideas" (Kohn 1969, p. xii). I take this occasion to re-affirm this fundamental tenet of my scientific faith. Its relevance to this essay is, I trust, obvious: In the process of speculating, testing, and speculating anew, cross-national research, properly employed, provides uniquely valuable evidence. There is no other evidence so useful for confirming social-structural interpretations, or for discovering their limitations. Either way, cross-national research is of pivotal importance for the development and testing of sociological theory.

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# THE POLITICAL PARTISANSHIP OF AMERICAN BUSINESS: A STUDY OF CORPORATE POLITICAL ACTION COMMITTEES\*

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*This study uses data on the contributions of corporate political action committees to evaluate six popular theories of business political partisanship. Two theories are supported by the data: the "Yankee-Cowboy" theory of regional political differences among U.S. corporations and the regulatory environment theory, which views the differential relationship to government regulation as a primary determinant of corporate political behavior. No support is found for four other theories of business political partisanship: the core-periphery theory, the inner-circle theory, the managerialist theory, and the domestic-multinational theory. The four disconfirmed theories are all variants of a perspective known as the theory of "corporate liberalism," which hypothesizes a tendency toward greater liberalism on the part of the more dominant or central corporations in American society.*

The political partisanship of American business has long been a topic of interest in both sociology and political science. Different schools hold conflicting theories of the nature of business political partisanship. Pluralists emphasize the divisions within the business community: policies that benefit one sector of business are often detrimental to others; hence corporations frequently find themselves opposed to one another in the political arena (Truman 1951; Dahl 1958; Ziegler 1964; Rose 1967; Ippolito and Walker 1980). Marxist and elite theorists see much more unity within the business community: fundamental interests in defending the system of private property and production for profit override whatever divisions exist among corporations. However, even Marxist and elite theorists make distinctions between the more conservative and more liberal sectors of the capitalist class (Domhoff 1972, pp. 109-70; 1978, pp. 117-19; Szymanski 1978, pp. 40-53). Numerous cleavages have been hypothesized to explain such political differences within the business community, including the division between big and medium-sized capital, domestic and multinational firms, and regional differences between North and South. Evidence demonstrating the significance of these divisions, however, is piecemeal and anecdotal.

New data on the activities of corporate political action committees (PACs) allow a more systematic investigation of this question. A corporate PAC is established by the directors or chief executive officer of a corporation and is legally affiliated with that corporation. A

corporate PAC solicits donations from company executives and stockholders and distributes that money to political candidates.<sup>1</sup> Corporate PACs are a relatively recent phenomenon on the American political landscape. Prior to 1971 it was illegal for corporations to make direct contributions to candidates for federal office. Companies wishing to contribute to political candidates were forced to do so under the table out of secret corporate slush funds or indirectly, by channeling money through individual owners or managers (Heard 1960, pp. 133-34). Between 1971 and 1976, however, a series of election law reforms, court decisions, and administrative rulings legally sanctioned the use of corporate funds to establish and administer political action committees (Cantor 1982, pp. 294-308). Since the mid-1970s, the number of corporate PACs has grown rapidly. In 1974 there were only 89 corporate PACs registered with the Federal Election Commission; by 1982 there were 1,467. Total expenditures of corporate PACs reached \$43.3 million in 1982. The largest share, \$27.5 million, went to candidates for the U.S. House and Senate, accounting for 8 percent of the total money spent during the 1982 Congressional election. Business-oriented trade association PACs accounted for another 6 percent of the total (Sabato 1984, pp. 10-16).

While the legalization of corporate PACs has facilitated a dramatic increase in corporate campaign spending, it has also provided the first systematic record of business political partisanship. The same laws that sanctioned corporate PACs also required them to file periodic reports of their income and expenditures with the

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<sup>1</sup> See Handler and Mulkern (1982, pp. 35-98) for a detailed discussion of the organization and operation of corporate political action committees.

Federal Election Commission. Every two years, beginning in 1978, the FEC has compiled a computerized list of all PACs, the candidates to which they have contributed, and the amount of those contributions.

These data on corporate PAC contributions have been used recently to explore several aspects of business political partisanship. Ashford (1986) has demonstrated a link between the magnitude of corporate campaign contributions and the success of candidates in the 1980 Congressional election. Several studies have shown the importance of corporate PACs in financing candidates of the ideological far right (Koenig and Boyce 1985; Burris 1987). Clawson, Neustadt, and Bearden (1986) have shown that, despite conflicting PAC strategies by different corporations in the 1980 election, the aggregate pattern of corporate campaign spending (measured by the consistency of business support for one or the other candidate in each Congressional contest) was one of remarkable unity. Also using 1980 data, Mizruchi and Koenig (1985, 1986) found that intraindustry integration and interindustry constraint were both conducive to corporate political cohesion as measured by the degree of overlap in candidates supported. However, apart from Clawson, Kaufman, and Neustadt (1985), who found little consistency in factors associated with corporate liberalism or conservatism in a sample of 66 ideologically polarized Congressional races in 1980, there has been no attempt to systematically analyze those characteristics of individual corporations that are associated with specific patterns of political partisanship. Such an analysis is the aim of this study.

#### THEORIES OF BUSINESS POLITICAL PARTISANSHIP

Probably the most frequently hypothesized political division within the business community is that between big business and small or medium-sized business (Monson and Cannon 1965; Baran and Sweezy 1966; Kross 1970; O'Connor 1973; Miller 1975; Lindblom 1977; Domhoff 1978; Useem 1980). We refer to this as the "core-periphery" theory of business partisanship. Adherents of this theory maintain that large, oligopolistic, capital-intensive, and/or relatively profitable firms confront different economic circumstances from smaller, competitive, labor-intensive, and/or less profitable firms, and that this is reflected in different patterns of political behavior. Core firms, adherents argue, are relatively insulated from the competitive pressures of the market, have a bigger stake in the long-term stability of the system, and are better able to protect their profit margins by passing on any increase in taxes or labor costs to their customers. For these reasons, they "tend to favor pro-

gressive labor and welfare legislation as a means of ensuring the stability of their own work force and, more generally, domestic tranquility" (Useem 1980, p. 61). Peripheral firms, by contrast, confront much more stringent economic constraints and are consequently more hostile to government regulation, taxation, unionization, and welfare spending that puts a floor under wages. Often this core-periphery theory of business partisanship is incorporated into a broader historical perspective, known as the theory of "corporate liberalism," which interprets the growth of the welfare state, state regulation of the economy, and other liberal reforms as products of enlightened efforts of the dominant sector of the business community to stabilize the capitalist order (Kolko 1963; Weinstein 1968; Domhoff 1978).

A closely related perspective is the theory of the corporate "inner circle" popularized by Useem (1978; 1984). Like the core-periphery theory, the inner-circle theory distinguishes between the more dominant or central firms and the more subordinate or peripheral firms. Rather than defining dominance or centrality in terms of size, market power, or capital intensity, however, the inner-circle theory classifies firms according to their prominence or centrality within intercorporate networks. Firms that are highly interconnected with other firms through director interlocks and participation in business policy formation groups are hypothesized to follow a political strategy more sensitive to the classwide interests of business as a whole, while less connected firms are hypothesized to follow a narrower, company-specific set of political priorities. Following this line of reasoning, Useem predicts that firms that are centrally located in the intercorporate network will be less opposed to liberal reform, regulation, and state intervention than firms on its margins. "Since welfare, labor, and other forms of government-managed reforms can be costly to individual firms but valuable to all if the reforms maintain societal stability, inner-group members are likely to be less opposed to state intervention in these realms than will be other capitalists" (Useem 1978, p. 228). "Sometimes termed 'corporate liberalism,' this attitude is rooted in . . . the recognition that the entire business community and the future of the private economy will best prosper if it assumes a posture of compromise. It is this rejection of a rigid opposition to everything that organized labor and government programs represent . . . that distinguishes the inner circle's views" (Useem 1984, p. 114).<sup>2</sup>

<sup>2</sup> An alternative interpretation of Useem's distinction between company-specific and classwide political rationality is suggested by Clawson et al. (1986). They argue that company-specific interests encourage contributions

A third perspective, which is also somewhat similar to the core-periphery theory, is the theory of "managerialism." Managerialists argue that the politically significant factor that distinguishes most large corporations from smaller firms is the separation of ownership and control. According to managerialist theory, the dispersal of stock ownership has resulted in a decline in the traditional power of capitalist owners. Consequently, an increasing number of large corporations have come under the control of professional managers who are not themselves major owners in their firms (Berle and Means 1967). Compared with traditional capitalist entrepreneurs, professional managers are hypothesized to be less committed to profit maximization and more responsive to a variety of other societal demands, including the interests of workers, consumers, and the community at large (Kaysen 1957, pp. 513-14; Gordon 1961, pp. 326-43; Galbraith 1967, pp. 314-24). Concerning state policy, professional managers are hypothesized to be more bipartisan (Galbraith 1967, p. 323), "closer to the political center," "less estranged from big government," and more "convinced of the necessity of the expanded role of government to prevent deep depressions and possible radicalism" (Monson and Cannon 1965, pp. 46-48).

A fourth theory of business partisanship emphasizes the division between domestic and multinational firms. According to this perspective, the conflict between "isolationist" and "internationalist" interests is the most enduring political cleavage within the business community. Domestic firms are hypothesized to favor protectionist tariffs and other means of restricting import competition, while multinational firms are hypothesized to support free trade as a means of increasing their access to foreign markets. To a degree, this cleavage also overlaps with that of the core-periphery model, since the largest firms tend also to be those with the most extensive foreign operations. In partisan terms, isolationist firms are usually seen as providing business support for the right wing of the Republican Party, while internationalist firms are viewed as being aligned either with the Democrats or, more commonly, with the "liberal" Eastern wing of the Republican Party (Crawford 1980, pp. 86-87; Friden 1980; Ferguson and Rogers 1981).

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to incumbents, regardless of party, as a means of buying influence on specific legislative issues of concern to the corporation. Classwide interests are interpreted as dictating a more ideologically conservative strategy of contributing heavily to right-wing challengers (thereby sacrificing company-specific access to liberal or moderate incumbents) in an effort to elect a more solidly "pro-business" Congress.

A fifth theory of business partisanship, commonly referred to as the "Yankee-Cowboy" theory, divides corporations along regional lines: eastern banks and midwestern manufacturing corporations are seen as moderate to conservative in political orientation, while rising Sun Belt firms in industries like defense, oil, agribusiness, textiles, and construction are viewed as more ultra-conservative (Sale 1975, pp. 89-152; Dye 1976, pp. 178-86). Adherents of the Yankee-Cowboy theory attribute this division to the greater conservatism of "new money" versus "old money" and the distinctive cultural climate of the American South and West (the legacy of slavery, religious fundamentalism, the frontier ethic, etc.). The Yankee-Cowboy theory has been popular in recent analyses of business support for Ronald Reagan and the New Right (Crawford 1980, pp. 78-110; Davis 1981, pp. 39-43).

A sixth theory of business partisanship argues that differences in corporate political behavior are primarily a reflection of different regulatory environments. According to this perspective, the major political cleavage within the business community is not one between conservatives and liberals but between the more ideological and more pragmatic firms (Handler and Mulkern 1982, pp. 7-34). From an ideological standpoint, most corporations prefer Republicans over Democrats and conservatives over liberals. Many corporations, however, temper this ideological conservatism with a pragmatic concern for maintaining access to influential incumbents, regardless of party or ideology. The firm's regulatory environment is seen as one of the most important factors influencing this trade-off between partisanship and pragmatism. Traditional regulated industries, with a longstanding and relatively cooperative relationship to industry-specific regulatory agencies and Congressional oversight committees (banks, utilities, airlines, etc.), are hypothesized to place the highest priority on retaining the good will of incumbents. Companies with a large percentage of their sales to government (e.g., major defense contractors) are likely to be swayed by similar considerations. Firms with less immediate interest in industry-specific regulation and contracts are hypothesized to be less constrained by the need to maintain access to incumbents and therefore freer to follow their ideological preference for conservatives, including Republican challengers of incumbent Democrats. Finally, firms that have had the most antagonistic relationship to government regulation—those that have been embroiled in the greatest conflict with the newer multi-industry regulatory agencies like the Environmental Protection Agency and the Occupational Safety and Health Administration—are hypothesized to be the most

willing to risk the displeasure of incumbents in order to elect a more Republican and conservative Congress. Corporate opposition to these new forms of multi-industry regulation has been cited by several authors as one of the most important causes of the generally conservative political mobilization of American business during the past decade (Edsall 1984, pp. 107-40; Useem 1984, pp. 160-71; Himmelstein and Clawson 1985).

These six perspectives on business political partisanship provide the theoretical framework for our study. This typology is oversimplistic in certain respects. For reasons of exposition, we have presented these as discrete and unidimensional theories. In practice, however, theorists of business political partisanship often draw upon a combination of these perspectives. Useem (1984), for example, combines aspects of the core-periphery theory with his own distinctive theory of the corporate inner circle. Yankee-Cowboy theorists like Davis (1981) cite such factors as labor intensity and the prevalence of entrepreneurial (rather than managerial) control to explain the conservatism of Sun Belt corporations. Because there are relatively few points on which these theories are directly contradictory, there are other, equally coherent, theoretical syntheses. By distinguishing somewhat artificially among these six perspectives, however, we hope to be better able to isolate and evaluate the different causal logics implicit in the literature in this field.

#### DATA AND MEASURES

The data used to evaluate these theories of business partisanship were taken from the Federal Election Commission computerized list of all PAC contributions in the 1982 Congressional election. For our sample we began with the 1,000 largest U.S. corporations in 1982. These included the *Fortune* 500 largest industrials, 100 largest diversified service companies, 50 largest banks, 50 largest insurance companies, 50 largest diversified financial companies, 50 largest retailers, 50 largest transportation companies, 50 largest utilities, and 100 largest private companies. Out of this group there were 443 corporations that made at least \$5000 in PAC contributions to Congressional candidates in the 1982 election. These 443 corporations accounted for 78 percent of all corporate PAC contributions to Congressional candidates in the 1982 election, and form the primary sample for our study.

For each corporation in this sample we constructed three different measures of political partisanship. The first was the percentage of total Congressional contributions to incumbents, the second was the percentage of total Congressional

contributions to Republicans, and the third was the percentage of total Congressional contributions to candidates of the New Right. A New Right candidate was defined as one who received contributions from at least three of the big five New Right ideological PACs. These are the National Conservative Political Action Committee, the Committee for the Survival of a Free Congress, the National Congressional Club, Citizens for the Republic, and the Fund for a Conservative Majority. Seventy-one candidates (mostly Republican nonincumbents) qualified as New Right by this criterion—about 8 percent of the total number of major-party candidates in the general election.

These three measures tap different but interrelated aspects of political partisanship. The percentage of contributions to Republicans versus Democrats is the measure most consistent with the commonsense meaning of "partisanship" and provides a rough index of political conservatism or liberalism. However, such a measure captures only part of the relevant variation in corporate political behavior. Contributing to a Republican incumbent with no serious opposition is not the same kind of partisan act as contributing to a Republican challenger in a closely contested race; nor is contributing to a Republican moderate like Charles Mathias the equivalent of contributing to a Republican archconservative like Jesse Helms. Additional measures are needed to capture these further dimensions of political partisanship.

The percentage of contributions going to incumbents is especially relevant to those theories that see concern for access to incumbents as the main moderating influence on corporate conservatism. Consistent with this view, corporations that contribute heavily to incumbents do appear to attach less importance to the party of candidates. There is a negative correlation between the percentage of contributions to incumbents and the percentage to Republicans ( $r = -0.51$ ).

Of the three measures, the percentage to New Right candidates is probably the most revealing. The New Right PACs, whose choices define these candidates, specifically seek to identify those races in which their money can have the greatest partisan impact—those races with a sharp ideological difference between the candidates and where additional funds can alter the outcome of a close contest. Corporations that contribute disproportionately to these same candidates can be assumed to be pursuing a similar political strategy, or, in some cases, to be directly following the example and advice of the New Right PACs (Sabato 1984, pp. 44-49). Because the choices of the New Right PACs are largely Republican nonincumbents, a high level

of support for New Right candidates is typically associated with a high level of support for Republicans ( $r = 0.57$ ) and a low level of support for incumbents ( $r = -0.82$ ).

For each of the 443 corporations in this sample we also collected a variety of data from business directories, company annual reports, and other sources. Each corporation was first classified into 1 of 28 major industrial groups on the basis of Standard Industrial Classification codes. To evaluate the core-periphery theory, we collected data on total sales, profit rate, capital intensity (assets per worker), and oligopoly power (using Tolbert, Horan, and Beck's [1980] index).<sup>3</sup> To evaluate the inner-circle theory, we ascertained whether the corporation was a member of the Business Roundtable (the business lobby most emphasized by Useem) and obtained data for each corporation on the number of director interlocks with a cross-section of 100 large U.S. corporations (U.S. Senate 1980). To evaluate the theory of managerialism we classified each corporation as manager-controlled, owner-controlled, or uncertain, based on the findings of previous studies (Burch 1972; Kotz 1978; Herman 1981; Corporate Data Exchange 1981). To evaluate the theory of domestic-multinational cleavage, we obtained data on the percentage of each corporation's total sales, assets, and profits generated abroad, and the degree of import competition (imports as a percentage of domestic production) in the industry where each corporation did the greatest share of its business (U.S. Department of Commerce 1983). To evaluate the Yankee-Cowboy theory, we dichotomized corporations into "Frost Belt" (Northeast and Midwest) and "Sun Belt" (South and West) categories according to the location of their corporate headquarters. To evaluate the regulatory environment theory, we constructed a three-point index. A corporation was coded +1 if it was in one of the traditional regulated industries (banking, insurance, finance, transportation, utilities, or drugs), -1 if it was in an industry with a high frequency of environmental and labor regulatory violations (using estimates by Clinard, Yeager, Brissett, Petrashek, and Harries [1979])<sup>4</sup> and 0 if it was in neither of these categories (there were no industries in both categories). To measure the impact of govern-

ment sales on political partisanship, we computed the percentage of defense contracts in the total sales of each corporation (from data in U.S. Department of Defense [1982]).

In addition to the 443 corporations included in this sample, there were another 698 PACs active in the 1982 Congressional election that were affiliated with companies too small to qualify among the 1,000 largest U.S. firms. For comparison, we also calculated the percentage of contributions to incumbents, Republicans, and New Right candidates for these 698 PACs. This enables us to contrast the political partisanship of the largest U.S. corporations with that of the remainder of small and medium-sized corporations with PACs, even though we lack the data to make finer distinctions among the latter group.

## FINDINGS

Multiple regression and discriminant analysis were our primary methods of assessing the impact of corporation characteristics on political partisanship. Before turning to the results of these analyses, however, we will examine the differences by industry in patterns of corporate PAC contributions. Table 1 shows the average percentage of contributions to incumbents, Republicans, and New Right candidates for each of 28 industry groups. As would be expected, the rankings of industries on these three measures of political partisanship are highly correlated. Industries that are high in contributions to incumbents tend to be low in contributions to both Republicans and New Right candidates. Spearman's  $r$  for the rank order correlation among industries according to their average on these measures is  $-0.73$  between incumbent and Republican contributions,  $-0.84$  between incumbent and New Right contributions, and  $0.82$  between Republican and New Right contributions.

The industry averages shown in Table 1 are generally most consistent with the regulatory environment and Yankee-Cowboy theories. For example, traditional regulated industries like drugs, transportation, and utilities are among the highest in the percentage of contributions to incumbents. The aerospace industry, with its concentration of major defense contractors, is also above average in contributions to incumbents. These same industries, together with banking, tend to be among the most bipartisan (i.e., lowest in the percentage of contributions to Republicans). And since New Right candidates are disproportionately Republican nonincumbents, several of these same industries rank among the lowest in New Right contributions. Conversely, industries with a high frequency of environmental and labor violations (e.g., chem-

<sup>3</sup> In the case of nonindustrial corporations, total revenues were treated as the equivalent of total sales.

<sup>4</sup> Clinard et al. (1979) identify the following industries as high in environmental and labor violations: chemicals, petroleum refining, paper and wood products, metal manufacturing, electrical equipment, and motor vehicles. To this cluster we added two additional industries noted for their hostility to environmental and labor regulation: mining and textiles.



Table 1. Contributions of Corporate PACs in the 1982 Congressional Election: Industry Averages

Industry (N)	Percent Incumbents	Percent Republican	Percent New Right
Mining, crude oil (18)	65.8*	70.0	24.8*
Construction (6)	57.5*	81.5	35.5*
Food, beverages, tobacco (36)	75.1	69.4	19.0
Textiles, apparel (7)	63.4	84.6*	37.9*
Paper, fiber, wood (20)	68.8	77.9*	19.8
Publishing, printing (4)	78.0	66.8	7.0
Chemicals (18)	68.8	78.2*	26.9*
Drugs (13)	86.2*	60.1	12.5
Soaps, cosmetics (5)	67.4	81.4	21.2
Petroleum refining (23)	66.7*	72.1	25.3*
Tires, rubber (4)	75.3	75.3	20.5
Stone, clay, glass prod. (12)	71.6	81.6*	22.3
Primary metal industries (20)	80.6	66.2	14.1
Fabricated metal products (8)	60.4*	72.9	21.5
Industrial, farm machinery (18)	63.4*	84.8*	27.1*
Electrical equipment (21)	71.5	74.4	20.1
Computers, office machines (9)	67.8	74.2	24.4
Motor vehicles and parts (10)	70.8	72.2	22.8
Aerospace (15)	82.9*	60.1	19.1
Other transportation equipment (4)	72.8	60.3	14.0
Transportation (32)	89.1*	47.3*	8.2*
Utilities (45)	79.0*	56.0*	13.8*
Wholesale trade (5)	59.0	80.0	27.6
Retail trade (17)	76.9	61.6	14.9
Banking (34)	74.3	57.3*	15.6
Insurance (10)	80.0	57.3	16.1
Other financial (17)	79.8	67.1	15.8
Miscellaneous (12)	69.3	53.5*	15.3
All industries (443)	74.2	66.9	18.7

\* Industry mean differs significantly from overall mean ( $p < 0.05$ ).

icals, oil refining, paper and wood products) are generally low in contributions to incumbents and high in contributions to Republicans and New Right candidates. As hypothesized by the Yankee-Cowboy theory, several of the industries highest in contributions to Republicans and New Right candidates are also industries in which Sun Belt firms are overrepresented. These include mining and crude oil (which is 67 percent Sunbelt in our sample), construction (100 percent Sunbelt), textiles (57 percent Sunbelt), and petroleum refining (65 percent Sunbelt). With the exception of petroleum refining, however, the conservatism of these industries might also be explained by the core-periphery theory, the inner-circle theory, or the domestic-multinational theory. One final industry that is noticeably more conservative than average is industrial and farm machinery. This industry is extremely diverse and not easily classified according to any of the six theories of business political partisanship.

A clearer picture of the pattern of business political partisanship emerges if we examine the relationship between corporate political behavior and the specific background characteristics of corporations emphasized by our six theories. Table 2 shows the regression and correlation

coefficients for 12 independent variables and each of the three measures of business political partisanship. The first entry in each cell of Table 2 is the unstandardized regression coefficient, the second is the standardized regression coefficient, and the third is the zero-order correlation coefficient. As in Table 1, there is a high degree of similarity in the patterns revealed by the three measures of political partisanship. Generally, independent variables that have a significant association with contributions to incumbents have a comparable (but opposite) association with New Right contributions. This suggests that it is concern over maintaining access and influence with incumbents, more than any other factor, that limits the support of corporations for candidates of the New Right. The percentage of contributions to Republicans is less consistently aligned with the other two measures of political partisanship. When an independent variable is significantly associated with Republican contributions, the sign of the relation is usually the same as that with New Right contributions and the opposite of that with contributions to incumbents.

The findings in Table 2 further support the Yankee-Cowboy and regulatory environment theories, while tending to disprove the claims of

Table 2. Determinants of Corporate PAC Contributions: Regression and Correlation Coefficients

Independent Variable	Dependent Variable		
	Percent Incumbents	Percent Republican	Percent New Right
Sun Belt location	-9.61 -0.28*** (-0.30)***	-3.10 -0.07 (-0.08)	7.76 0.30*** (0.30)***
Regulatory environment	6.25 0.30*** (0.27)***	-9.00 -0.36*** (-0.34)***	-4.95 -0.32*** (-0.31)***
Defense contracts (% sales)	0.23 0.14** (0.12)**	-0.28 -0.15** (-0.10)*	-0.15 -0.13** (-0.10)*
Size (total sales)	0.00 -0.07 (0.03)	0.00 -0.03 (-0.02)	0.00 0.02 (-0.03)
Profit rate	-0.29 -0.12** (-0.12)**	0.55 0.19*** (0.17)***	0.24 0.13** (0.13)**
Oligopoly index	-0.02 -0.02 (-0.07)	0.02 0.02 (0.14)**	0.03 0.04 (0.11)*
Capital intensity	0.00 0.02 (0.10)*	0.00 -0.01 (-0.13)**	0.00 -0.02 (-0.11)*
Director interlocks	0.25 0.07 (0.13)**	-0.30 -0.07 (-0.03)	-0.17 -0.07 (-0.11)*
Business Roundtable member	1.69 0.05 (0.09)	0.26 0.01 (0.04)	-0.17 -0.01 (-0.02)
Manager controlled	-0.17 -0.01 (0.04)	3.11 0.08 (0.07)	0.79 0.03 (0.00)
Foreign operations	0.04 0.04 (0.08)	0.12 0.09 (0.11)*	0.03 0.04 (0.00)
Import competition	0.04 0.03 (-0.15)**	-0.09 -0.07 (0.12)**	-0.03 -0.04 (0.17)***
Constant	77.90	62.60	13.52
Multiple R	0.45	0.43	0.47
Multiple R <sup>2</sup>	0.21	0.19	0.22
Total N	443	443	443

Note: The top entry in each cell is the unstandardized regression coefficient. The second entry is the standardized regression coefficient. The third entry (in parentheses) is the zero-order correlation.

\*  $p < .05$ .

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .

the other theories. Consistent with the Yankee-Cowboy theory, Sun Belt location is positively associated with support for nonincumbents and New Right candidates. When Sun Belt firms do contribute to incumbents, however, there is a slight (statistically insignificant) tendency for these to be disproportionately Democrats. Presumably, this reflects the predominance of Democratic incumbents in parts of the South and the tendency for firms to contribute most

frequently to those incumbents with whom they have a constituency relationship.

Consistent with the regulatory environment theory, the regulatory environment index is positively associated with bipartisanship and support for incumbents and negatively associated with support for the New Right. This indicates that the traditional regulated industries are above average in contributions to incumbents and below average in contributions to

Republicans and New Right candidates, while industries with the most antagonistic relation to environmental and labor regulation have just the opposite characteristics. The variable for percentage of defense sales follows a similar pattern: defense contractors are relatively high in their support for incumbents and relatively low in their support for Republicans and New Right candidates.

None of the other theories of business partisanship are consistently supported by the data in Table 2. Contrary to the core-periphery theory, large firms (measured by sales) are no more liberal than medium-sized firms. The more profitable and more oligopolistic firms are, if anything, more conservative than the less profitable and more competitive firms. The only pattern consistent with the core-periphery theory is the fact that capital-intensive firms are slightly more liberal than labor-intensive firms at the zero-order level. This is mainly an artifact of regional differences in capital-intensity and the greater bipartisanship of such capital-intensive regulated industries as banks and utilities. Hence, the capital-intensity variable does not add significantly to the explained variance once these other variables are entered into the regression.<sup>5</sup>

The inner-circle theory receives some limited support in that firms that are heavily interconnected through director interlocks are slightly more moderate as measured by their low support for New Right candidates. However, this zero-order correlation is mainly a reflection of the fact that the most heavily interconnected firms (banks and insurance companies) are also among the traditional regulated industries. The director interlocks variable does not remain significant once the regulatory environment variable is included in the regression equations. Also contrary to the inner-circle theory, firms that are members of the Business Roundtable are not significantly different from nonmembers in their political contributions.

Contrary to the theory of managerialism, manager-controlled firms are no more liberal or bipartisan than owner-controlled firms.

Finally, contrary to the theory of domestic-

multinational cleavage, foreign operations (measured by an average of the proportion of sales, assets, and profits generated abroad) have no strong effects on political partisanship. At the zero-order level, vulnerability to import competition is associated with support for the Republican right wing as predicted by this theory. But this correlation appears to be an artifact of the greater import competition experienced by Sun Belt firms and the lesser import competition facing traditional regulated industries, many of which are natural monopolies. Accordingly, the effects of import competition do not remain significant once the regulatory environment and Sun Belt variables are included in the regression equations.

As an alternative test of the different theories of business political partisanship, we conducted a discriminant analysis on the top and bottom quintiles of each dependent variable using the same group of independent variables as discriminating variables. The literature on this topic frequently focuses on the factors thought to distinguish those firms which occupy the liberal or conservative extremes of political partisanship, rather than on the variation among corporations in the middle range. To test this interpretation of the contending theories of business political partisanship, we selected those firms that were in the top and bottom quintile according to each of the three measures of political partisanship and examined how well each of our 12 independent variables was able to discriminate between these two groups. The results of this analysis are shown in Table 3. Because linear discriminant analysis is the equivalent of multiple linear regression when the dependent variable is binary, we have taken the corresponding standardized regression coefficient as a summary measure of the relative discriminating power of each independent variable. Only four variables are significant (at the  $p < 0.05$  level) in discriminating between the two extremes of political partisanship: Sun Belt location, regulatory environment, defense contracts, and profit rate. The pattern for the first three variables is consistent with the Yankee-Cowboy and regulatory environment theories, while the sign on the fourth variable is in the opposite direction of that predicted by the core-periphery theory. The discriminant function constructed from these four variables correctly classifies between 76 and 78 percent of the firms into the appropriate quintile of political partisanship.

Before we dismiss the popular core-periphery theory of business partisanship, a final comparison is necessary. Advocates of the core-periphery model might argue that our sample does not allow for a complete test of their theory. At most we have shown that there are

<sup>5</sup> Out of concern for the possible incommensurability of certain core-periphery variables across different types of firms, we conducted parallel analyses using a sample restricted to industrial corporations only and a sample restricted to nonfinancial corporations only. The results of these analyses were similar to those reported here. We also tried several alternative measures of corporation size (assets, employees, etc.) and a dichotomous core-periphery industrial classification instead of the linear oligopoly index. None of these alternative measures produced any stronger support for the core-periphery theory.

Table 3. Variables Discriminating Between High and Low Quintiles of Political Partisanship: Standardized Regression Coefficients

Independent Variable	Dependent Variable		
	Percent Incumbents	Percent Republican	Percent New Right
Sun Belt location	-0.40***	-0.09	0.39***
Regulatory environment	0.38***	-0.47***	-0.40***
Defense contracts (% sales)	0.13*	-0.15*	-0.09
Profit rate	-0.14*	0.16*	0.11
Multiple $R^2$	0.34	0.29	0.33
Proportion of cases correctly classified	0.78	0.76	0.76
Total $N$	183	186	188

\*  $p < .05$ .\*\*\*  $p < .001$ .

few political differences between large and medium-sized corporations; however, it is possible that the most significant political cleavage may be found between these and even smaller firms. To test for this possibility, we compared the partisanship of those firms in our original sample (those which ranked among the 1,000 largest U.S. corporations) with that of the remaining 698 PACs belonging to smaller corporations. The results also fail to support the core-periphery theory. There is a modest tendency, we found, for smaller corporations to be more generous in their contributions to challengers. In aggregate terms, the smaller firms contributed 69.8 percent of their PAC dollars to incumbents, compared with 74.5 percent for the larger firms. This seems to indicate a lesser concern of smaller firms with maintaining access to incumbents and a greater propensity to make contributions based on ideology. Were this combined with a greater tendency to support either Republicans or New Right candidates, the core-periphery hypothesis of greater conservatism of peripheral firms would be confirmed. Actually, the smaller firms were somewhat more likely to support Democrats and virtually identical in their support for New Right candidates. Smaller firms gave 62.5 percent of their PAC dollars to Republicans, compared with 66.5 percent for larger firms. Contributions to New Right candidates were 19.6 percent of the PAC contributions of smaller firms, compared with 19.0 percent for larger firms. Thus, while there are modest differences in the PAC behavior of smaller and larger firms, these are not the sort predicted by the core-periphery theory.<sup>6</sup>

<sup>6</sup> We also examined the question of whether larger firms, even if they are not more liberal than smaller firms, might at least be more uniform in political partisanship. In other words, are smaller firms more likely to be found at the extremes of political partisanship, even though their average partisanship is comparable to that of larger firms? We found little

It is still possible, of course, that the political differences predicted by the core-periphery theory may occur at an even lower point on the corporate scale. We cannot rule out the possibility that firms that are typically too small to operate PACs may be more conservative than those that do. If corporation size, dominance, and centrality are important dimensions of business political partisanship, however, it seems unlikely that differences along these dimensions would not be found, at least weakly, among the corporations in our sample.

#### DISCUSSION AND SUGGESTIONS FOR FURTHER RESEARCH

This study provides support for two theories of business political partisanship—the Yankee-Cowboy theory and the regulatory environment theory—but fails to support four other popular theories. The four disconfirmed theories—core-periphery, inner-circle, managerialist, and domestic-multinational—overlap with one another and share a similar explanatory logic. While they operationalize their terms in different ways and hypothesize slightly different mediating mechanisms, each argues that it is the more central or dominant firms that tend to be politically moderate or liberal. Such firms are hypothesized to be more favorable or tolerant toward state economic intervention and more willing to compromise immediate economic interests for the sake of the long-term stability and legitimacy of the system. The different versions of this argument may be classed together as variants of a common perspective,

support for this hypothesis either. The variance on each of the three measures of political partisanship is approximately 1.3 times as large for smaller firms as for larger firms; however, this is well within the range that would be expected simply from the fact that the partisanship measures for smaller firms are aggregated across a much smaller number of individual contributions.

called the theory of corporate liberalism. Our study supports none of the four variants of this theory. Indeed, the few significant effects that can be attributed to variables highlighted by these theories (e.g., profitability) are in the opposite direction than predicted.

There are several possible interpretations of these findings. On the one hand, it is possible that the theory of corporate liberalism may have been valid in earlier historical periods, but is no longer true today. For example, it may be that dominant business elites assume a more moderate or compromising political stance only when confronted with a powerful anti-corporate movement, such as that of the 1930s, which they seek to coopt. By this logic, we should not expect to find any evidence of corporate liberalism in the early 1980s, a period of unparalleled business hegemony in the political sphere. Alternatively, it may be the case that corporate liberalism is restricted to periods of relative economic prosperity and stability, such as the early post-World War II era, and that the economic shocks of the last decade and a half have resulted in a more defensive and conservative political orientation of dominant business elites. This interpretation is consistent with those theories that view the 1970s as a period of the breakup of the postwar Keynesian coalition that formerly accommodated the conflicting interests of big business, organized labor, and the welfare state (Gold 1977; Bowles, Gordon, and Weiskopf 1983).

On the other hand, it is possible that the theory of corporate liberalism may never have been accurate and that it is based entirely on false generalizations from the political behavior of individual business elites (Block 1977). Dominant firms provide a disproportionate share of corporate elites who become politically influential, so it is not surprising that the most visible business supporters of liberal reforms tend to be from dominant firms. It would be mistaken, however, to infer backward from these individual elites to the dominant business sector from which they come. To cite a contemporary example, the fact that individual representatives of big business can be found today who support a more aggressive policy of state intervention in the economy (New York investment banker Felix Rohatyn is often mentioned in this context) does not mean that big business in general supports such a policy, as numerous studies have shown (Vogel 1978; Wachter and Wachter 1981; *Business Week* 1983). Much of the evidence for the theory of corporate liberalism is based on just this kind of selective inference from the politics of individual corporate elites to the characteristics of broader business sectors.

The theory most strongly supported in this

study is the regulatory environment theory. Our results highlight the degree to which corporate political behavior is conditioned by the fiscal and regulatory relationship between corporations and the state. They are also consistent with those who argue that the desire to shape and control the regulatory process is one of the most important factors motivating the recent increase in business political activism (Edsall 1984; Useem 1984; Himmelstein and Clawson 1985).

These findings pose several questions for further research. For example, should we accept the simple three-fold typology of regulatory environments proposed in this study, or can this typology be further refined? Judging from Table 1, the classification of traditional regulated industries (banking, insurance, finance, transportation, utilities, and drugs) as a cluster with similar political tendencies appears to be well supported. The bias toward bipartisanship and support for incumbents is relatively uniform across these industries. Whether the grouping of industries with the most antagonistic relation to environmental and labor regulation forms an equally coherent cluster is less certain. Included in this cluster are mining, textiles, paper and wood products, chemicals, petroleum refining, primary metal industries, electrical equipment, and motor vehicles. Several industries in this cluster are indeed among the least pragmatic (as measured by their low support for incumbents) and most conservative (as measured by their high support for Republicans and New Right candidates) of all industries. Nevertheless, the variation within this cluster is greater than within the cluster of traditional regulated industries. Further research is needed to determine whether antagonism to government regulation is indeed the common factor underlying the political conservatism of these industries and to develop more refined indices for measuring such antagonism.

It would also be interesting to know whether the bipartisanship of the traditional regulated industries and their tendency to shun New Right candidates are merely tactical concessions to their greater need for access to incumbents or reflective of a genuine difference in political ideology. Our impression is that the truth lies somewhere in the middle. It is plausible that a longstanding and relatively cooperative relationship to government might instill a more liberal or moderate political outlook. On the other hand, examples can be cited of corporations in regulated industries that follow a pragmatic strategy of bipartisan support for incumbents with their PAC contributions, while simultaneously funding the most ultraconservative groups and causes outside of the electoral arena (Burris 1987). More systematic research on

other aspects of business political partisanship (philanthropic contributions, political activities of corporate owners and managers, etc.) is needed before any conclusions can be drawn on this question.

Perhaps the most surprising result of this study is the evidence we found for the Yankee-Cowboy theory of business political partisanship. While this theory is often given credence in journalistic accounts of American politics (Sale 1976), empirically inclined social scientists have tended to view the theory with disdain (Clawson et al. 1985, p. 64). Consequently, the theory has seldom been subjected to systematic quantitative investigation (see Johnson [1976] for a notable exception). The political differences we found between regions are not great, and some assumptions of the Yankee-Cowboy theory (e.g., the greater conservatism of defense contractors) are not supported. Nevertheless, the evidence of regional differences in business political partisanship is sufficiently impressive to warrant further investigation.

The first question that needs to be asked is how accurately does the simple Sun Belt/Frost Belt dichotomy capture the pattern of regional variation in business political partisanship? Are there further cleavages *within* these two regions? Using the percentage of contributions to New Right candidates to measure political partisanship and a more detailed regional breakdown, we found the lowest level of New Right support among corporations based in New England (13.4 percent), and roughly equal levels for the Mid-Atlantic States (17.0 percent) and the Midwest (16.9 percent). Within the Sun Belt, support for New Right candidates was highest in the mountain and Pacific states (24.7 percent). Interestingly, with the exception of North Carolina and the oil-rich states of Texas, Louisiana, and Oklahoma, which averaged 23.9 percent, corporations in the rest of the Deep South were *not* disproportionately supportive of New Right candidates (they averaged 17.2 percent). It is possible, therefore, that the Sun Belt/Frost Belt dichotomy may be an oversimplified view of regional variations in business political partisanship.

Questions also remain concerning the factors responsible for the greater conservatism of Sun Belt firms. Is this pattern explainable in terms of the distinctive industrial characteristics of the South and West or is something more involved—e.g., distinctive political institutions or cultures? The fact that the Sun Belt variable remains a significant predictor of political partisanship after controlling for numerous industrial characteristics strongly suggests the latter explanation. Similar results are achieved if

we transform the 28-industry breakdown in Table 1 into dummy variables and enter these into regression equations along with the Sun Belt variable. Controlling for the effects of industry in this fashion, the Sun Belt variable remains a significant predictor of contributions to both incumbents and New Right candidates ( $p < 0.0001$ ). This indicates that the political differences between Sun Belt and Frost Belt firms are caused by something more than the different industrial mix between regions.

It is tempting to view the conservatism of Sun Belt firms as merely a reflection of the more conservative political attitudes characteristic of the general populace in this region. If this were so, however, we would expect a more uniform tendency toward conservatism across the southern states. This interpretation is also difficult to square with the fact that, for example, corporations based in San Francisco contribute the same share of their PAC dollars to New Right candidates as corporations based in Houston or Dallas. Another explanation favored by Yankee-Cowboy theorists is the reputed difference in political outlook between "new money" and "old money." Ranking corporations according to the year in which they were founded, we were unable to find any political differences between newer and more established firms. To adequately test this hypothesis, however, we would need more detailed biographical information on the owners and managers of each corporation, since the new money/old money distinction is more appropriately applied to individual business elites than to the corporations with which they are associated. Further research is therefore needed to clarify the underlying causes of regional differences in business political partisanship.

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# LIMITS OF ETHNIC SOLIDARITY IN THE ENCLAVE ECONOMY\*

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*Contemporary research on the social and economic adaptation of immigrants to life in the United States emphasizes the salience of ethnic solidarity. Portes and others advance the "enclave-economy hypothesis" that immigrants in an enclave-labor market receive earning-returns to human capital commensurate with the earning-returns of immigrants in the primary labor market. This position contradicts the classical assimilation view that segregation retards the economic achievement of minorities. However, our analysis of earnings among Cuban and Chinese immigrants suggests that the enclave-economy hypothesis is only partially correct. The hypothesis is supported in the case of entrepreneurs, but the assimilation perspective better explains the earnings of employees. We suggest a reformulation of the enclave-economy hypothesis that is sensitive to important differences between immigrant-workers and immigrant-bosses.*

In the early part of this century, scholars sought to identify recurrent patterns in the processes through which waves of diverse European immigrant groups settled into U.S. society (e.g., Handlin 1951). The view that there exists a universalistic process of adaptation extends to non-Europeans such as Asian immigrants to the west coast (Park and Burgess 1921). There have always been alternative and less universalistic interpretations (e.g., Bogardus 1930), but the broadly formulated position of Park and Burgess (1921) and Park (1926) is the most enduring sociological view of immigrant-adaptation. The four-part immigrant-adaptation/race-relations cycle of "contacts, competition, accommodation, and eventual assimilation" was viewed by Park (1926, p. 196, emphasis added) as a natural history of intergroup contact. This model assumed an evolutionary perspective, though the process of assimilation varied considerably in timing for different groups, depending on their

initial characteristics and on historical conditions shaping the response of the host society (see Hirschman 1983).

In Park and Burgess' view, assimilation was driven by social-psychological processes. Changes in social-structural relations (e.g., a decline in occupational discrimination) that open new avenues of immigrant-adaptation stem from intergroup mixing and the sharing of experiences. These developments are inevitable once intergroup contact is established. Conflict will probably precede the emergence of cooperative social arrangements, but, as groups come to share common definitions of various situations, the social-structural barriers between them are undermined. The *ecological hypothesis* of an inverse relationship between segregation and socioeconomic achievement among minority groups follows from this argument.

## OVERCOMING THE COSTS OF SEGREGATION?

Sociological study of the processes through which immigrants adapt to their new country is undergoing a marked revival.<sup>1</sup> Segmented-labor-market and ethnic-solidarity theories have emerged as the most influential in current studies of immigrant-incorporation. The segmented-labor-market perspective extends "dual economy" theory (Averitt 1968; Galbraith 1971) to the organization of labor markets. According to this view, the labor force in advanced capitalism

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<sup>1</sup> This revival seems to be largely in response to the upsurge of immigration from Asia and Latin America. Since the 1950s, the percentage of legal immigrants to the U.S. who hail from Latin America and East Asia has grown from less than 30 percent to approximately 65 percent (Statistical Abstract of the U.S. 1986).

is segmented into two or more labor markets (Gordon 1972; Edwards 1975). The primary labor market, because it is characterized by stable work conditions, higher wages, scarce skill specifications, and internal labor markets that provide ladders of success within the firm, provides higher returns on human capital investments for workers. By contrast, the secondary labor market is typically characterized by high turnover rates, low-paying, low-skill jobs that lack structured opportunities for promotion within the firm; it generates low returns on human capital investments. Segmented-labor-market theory posits that the dynamics of labor market incorporation in advanced capitalism result in the disproportionate concentration of racial minorities and women into the secondary labor market. Advanced capitalism also required a continual flow of low-wage, relatively unskilled immigrant laborers to fill undesirable jobs (Burawoy 1976; Sassen-Koob 1978; Piore 1979). Due to barriers to mobility between the secondary and primary labor markets, immigrant-minority groups become trapped in a succession of low-wage and unstable jobs (Bluestone 1970; Doeringer and Piore 1971). The assumption of eventual assimilation may not extend to some immigrant-minority groups.

A separate but related set of structural theories emphasizes the salience of ethnic solidarity in explaining the socioeconomic attainment of some racial and ethnic minorities despite persistent discrimination. Ethnic solidarity theorists focus on the institutions and social dynamics that facilitate the mobilization of ethnic resources for economic advancement. In explaining the high participation in small business of Chinese and Japanese immigrants, Light (1972) argues that a cultural proclivity towards business partnerships and the institution of rotating credit unions enabled Chinese and Japanese immigrants to start up small businesses. Some scholars (Siu 1952; Bonacich 1973) contend that the sojourning pattern of migration of some immigrant groups gives rise to an orientation that encourages reactive solidarity and economic activity associated with "middlemen minorities." In a case study of the Japanese-American community before World War II, Bonacich and Modell (1980) point to interactions between ethnic solidarity, small business concentration, and societal hostility that facilitated the mobilization of ethnic resources for economic action. Ethnic-solidarity theorists share an emphasis on ethnic resources to explain why some immigrant-minority groups achieve economic success despite societal hostility and initial disadvantages.

Building on both the segmented-labor-market and ethnic-solidarity theories, Alejandro Portes

and his colleagues (Portes et al. hereafter) have strongly challenged assimilation theory, particularly the ecological hypothesis. Portes et al. (e.g., Portes, Clark, and Bach 1977; Bach 1980; Portes and Bach 1980, 1985; Portes, Parker, and Cobas 1980; Wilson and Portes 1980; Bach, Bach, and Triplett 1981; Portes 1981, 1982, 1984; Portes, Clark, and Lopez 1982; Wilson and Martin 1982; Portes and Stepick 1985; Portes and Manning 1986) contend that some immigrant-minority groups avoid the harsh consequences of incorporation into the secondary labor market through the establishment of immigrant-enclave economies. Wilson and Portes (1980, p. 302) formalized this argument in the *enclave-economy hypothesis*, which they claim "directly contradicts conventional predictions:"

Immigrant workers are not restricted to the secondary labor market. In particular, those inserted into an immigrant enclave can be empirically distinguished from workers in both the primary and secondary labor markets. Enclave workers will share with those in the primary sector a significant economic return to past human capital investments. Such a return will be absent among those in the "open" secondary labor market.

Portes (1981, p. 291) defines the enclave as consisting of "immigrant groups which concentrate in a distinct spatial location and organize a variety of enterprises serving their own ethnic market and/or the general population. Their basic characteristic is that a significant proportion of the immigrant work force works in enterprises owned by other immigrants." Enclave economies can vary in size and division of labor. Within large and highly differentiated enclaves, immigrants go about their work and leisure activities without having to know the language of their host society and without extensive interactions outside of their ethnic group.

What renders the enclave economy hypothesis a critical challenge to the ecological hypothesis of the assimilation school is the proposition that, despite the social isolation of the enclave, there is no cost to segregation. Enclave workers, in contrast to workers in the secondary labor market, gain similar returns on human capital investments as workers in the primary labor market. The enclave-economy hypothesis advances a "separate but equal" proposition, which, if true, undermines a major tenet of assimilation theory.

According to the proponents of the enclave-economy hypothesis, immigrant entrepreneurs mobilize ethnic solidarity to establish a "unified system of vertical and horizontal integration," enabling enclave firms to replicate some of the economies of scale associated with core monop-

olistic firms. Hence, "the enclave resembles the center economy and should have many of the advantages which that form of economy enjoys" (Wilson and Martin, 1982, p. 138). Moreover, ethnic solidarity "serves to provide entrepreneurs with privileged access to immigrant labor and to legitimize paternalistic work arrangements" (Portes, 1981, p. 291), giving enclave firms favorable work discipline, and rendering them less vulnerable to unionization. Ethnic networks can also be drawn upon to generate informal sources of capital formation and captive markets, making enclave firms more self-sufficient and resilient. These factors also make the prospect of starting up firms more open to aspiring entrepreneurs. Enclave-economy proponents argue that ethnic solidarity involves "reciprocal obligations," which explain why enclave workers experience returns on human capital investment similar to workers in the primary labor market.

If employers can profit from the willing self-exploitation of fellow immigrants, they are also obliged to reserve for them those supervisory positions that open in their firms, to train them in trade skills, and to support their eventual move into self-employment. It is the fact that enclave firms are compelled to rely on ethnic solidarity and that the latter "cuts both ways," which creates opportunities for mobility unavailable in the outside" (Portes and Bach, 1985, p. 343).

But how strong is the evidence that ethnic solidarity encourages the mobilization of economic resources such that much of the economic cost traditionally associated with immigration and segregation is avoided? Studies of enclave economies often omit a comparison of opportunities and rewards across the boundaries of the enclave. An exception is Bonacich and Modell (1980), who found that, as opportunities became available to Japanese-Americans in the open economy, their participation in the enclave economy declined because rewards were greater in the open economy. It is possible that the economic rewards of enclave economies only appear to be favorable when comparisons to the outside economy are not made. Ethnic enclaves provide immigrant groups with a base of social support and cohesion that is crucial for those who are slow to acquire the language and cultural skills of the host society. But as long as immigrants lack the cultural and language skills of the host country, their employment opportunities are usually limited to relatively poor paying jobs. Residential segregation and regional concentrations of minorities have, in the past, aggravated the extent to which minority workers receive undesirable jobs and poor wages (Blalock 1956, 1957; Duncan and

Lieberson 1959; Lieberson 1963; Frisbee and Neidert 1977; Parcel 1979).

Empirical examinations of the enclave-economy hypothesis (Wilson and Portes 1980; Portes 1982; Portes and Bach 1985) reveal that in Miami, the enclave-labor market provides Cuban immigrants with earning-returns to human-capital characteristics commensurate with those achieved by Cuban immigrants employed in the primary labor market. Cubans employed in the secondary labor market fare more poorly. At face value, the evidence appears to confirm that segregated ethnic enclaves can provide recent immigrants with comparatively favorable rewards in the labor market. However, two problems should be resolved before accepting this evidence. First, both entrepreneurs and employees are pooled to create a sample of immigrants defined as "workers" in the enclave economy. In early analyses (Wilson and Portes 1980), approximately one-quarter of the enclave-economy labor force was self-employed Cuban immigrants. In later analyses (Portes and Bach 1985), over 40 percent were self-employed. No self-employed Cubans were included in either the primary or secondary labor-market subsamples. It is possible that self-employment and private-sector employment tend to be systematically associated with different levels of returns to a variety of human-capital characteristics. Considerable evidence suggests that immigrants both in and out of ethnic-enclave economies who operate small businesses fare substantially better in social and economic experiences than immigrants employed in the private sector (Bonacich, Light, and Wong 1977; Bonacich and Modell 1980; Kim 1981; Cobas 1985; Nee and Sanders 1985).

Portes et al. recognize that self-employed Cuban immigrants in Miami have several advantages over Cuban immigrants in the private sector of the enclave economy. For example, Portes and Bach (1980) report that employment in the Cuban enclave is directly associated with greater than a \$100-per-month earnings-disadvantage. This is substantial; Cubans averaged only \$647. In *Latin Journey*, Portes and Bach (1985) report significant differences between workers and bosses in the Cuban enclave (pp. 205-16), but they inexplicably ignore such differences when estimating returns to human capital. Because a discriminant analysis conducted on the sample as of 1979 indicates that the number of employees in a firm discriminates between labor markets even when self-employed Cubans are omitted, Portes and Bach (1985) may have felt justified in ignoring boss-worker comparisons when specifying their earnings equation. But the practice of not controlling for self-employment originated in an earlier study (Wilson and Portes 1980), in which

the discriminant analysis does not control for self-employment or the number of employees in a firm. Portes et al. do not provide a theoretical or empirical justification for pooling employees and entrepreneurs. Consequently, we cannot be sure to what extent their findings are problematic. Though small sample sizes may have ruled out a complete analysis of covariance, this would not have precluded the specification of a boss versus worker dummy variable or theoretically interesting interactions between the dummy variable and work experience, education, or other key independent variables.

Our second concern is that the earning-returns of recent immigrants who participate in the enclave economy are compared only to Cuban immigrant-workers in the Miami enclave who are defined as not participating in the Cuban controlled economy. This is a severely limited view of the "open" economy. Such a comparison is not a legitimate test of the fundamental ideas of the segmented-labor-market literature or assimilation theory. The research of Lieberman (1963), Piore (1979), and Bonacich and Modell (1980) suggests that ethnic-enclave workers may commonly receive lower rates of returns to human capital than ethnic workers who participate in the "outside" labor market. Though it is often the offspring of immigrants who first enter the mainstream economy on a large scale, the benefits of such employment are not restricted to U.S.-born ethnics.

In light of these concerns, the evidence in support of the hypothesis that immigrant-workers in an enclave economy are protected from the disadvantages associated with the "open" secondary labor market is inconclusive. It might be that immigrant-workers in ethnic-enclave settings do achieve approximately the same earning-returns to past human capital investments as immigrant-workers outside the enclave economy, but the research of Portes et al. is unable to demonstrate this. The two questions we have raised must be answered before we can feel confident that the ethnic-enclave hypothesis is supported. Both questions may be answered empirically.

#### EXAMINING THE MERITS OF THE ENCLAVE ECONOMY HYPOTHESIS

To test the enclave-economy hypothesis, we focus on the Cuban enclave in the Florida cities of Miami and Hialeah and on the Chinese enclave in San Francisco, California. Our study is a comparative examination involving two immigrant-minority groups. Given the arguments behind the enclave-economy hypothesis, we consider the usefulness of human-capital theory, the segmented-labor-market literature, and assimilation theory in helping us understand

how contemporary immigrants to the U.S. are adapting to their new home. If the enclave-economy hypothesis holds, we should find that the earning-returns to human-capital characteristics among immigrants in their respective enclave economies are *not* disadvantaged relative to the returns achieved by immigrants of the same ethnicity who possess equal human-capital characteristics but who live outside of the ethnic enclave.

Our strategy differs from Portes et al. in some important respects. First, we utilize the 1980 Census five percent public use micro-sample for Florida and California (U.S. Department of Commerce 1983a). These data represent self-reports obtained through mailout questionnaires. Second, the specification of our earnings equation utilizes the human-capital earnings function. Because the enclave-economy hypothesis focuses on returns to human capital, it is logical to use the human capital earnings function, since the model was designed to examine hypotheses drawn from human-capital theory. The human-capital earnings function, developed by Becker and Chiswick (1966) and modified by Mincer (1974), appears to be superior to alternative specifications used by economists and sociologists alike (Heckman and Polachek 1974; Petersen 1985a). Chiswick (1977a, 1977b, 1978, 1980) has made extensive use of the model in studying immigrants.

In 1980, Miami and Hialeah had a combined population of just under 500,000, approximately 50 percent of which was Cuban. While the Cuban business community is active throughout Dade county and beyond, Cuban enterprise, employment, place of residence, and cultural events are most concentrated in Miami and Hialeah. Boswell and Curtis (1984, Chapter 5) describe the Cuban community in South Florida. San Francisco's population in 1980 was almost 680,000, 12 percent of which was Chinese. Table 1 provides data on Cuban-owned businesses in the Miami SMSA and Chinese-owned businesses in the San Francisco-Oakland SMSA. In 1977, mean receipts for Cuban firms were \$85,000 compared to \$92,000 for Chinese firms. Though manufacturing firms tended to employ the most workers per firm in both the Cuban and Chinese enclave economies, the proportion of total receipts accounted for by various industries differed considerably between the two enclaves. Manufacturing accounted for the largest share (30 percent) of receipts among Cuban firms, while retailing accounted for 61 percent of all receipts among Chinese firms. The retail and service categories alone accounted for 75 percent of the total receipts of Chinese firms. Common to both enclave economies are the large numbers of garment shops (which tend to be heavy employers of women), retail shops,

Table 1. Composition of Ethnic-Owned Business Enterprises

Sector	# of Firms	% of Firms	% of Firms with Employees (Within Sector)	Average # of Employees among Firms with Employees	Total Receipts (Million \$)
<i>A. 1977 Cuban-owned firms in the Miami SMSA</i>					
Construction	941	13	14	5	40.21
Manufacturing	270	4	56	35	186.62
Transportation & public utilities	643	9	8	11	35.79
Wholesale	326	4	34	6.5	119.00
Retail	1,538	21	24	4	134.14
Finance, insurance, & real estate	441	6	10	10	23.71
Service	2,795	38	16	4.5	76.96
Other	382	5	9	—	5.94
Totals	7,336				622.37
<i>B. 1982 Cuban-owned firms in the Miami SMSA</i>					
Agriculture, forestry, fishing, & mining	442	2	7	2.5	7.54
Construction	2,181	11	8	5	87.52
Manufacturing	279	1	46	21	90.22
Transportation & public utilities	1,328	6	5	—	65.66
Wholesale	657	3	24	6	181.37
Retail	3,055	15	21	5	365.82
Finance, insurance, & real estate	1,348	7	11	4.2	55.74
Service	9,317	45	13	3	319.88
Other	2,188	11	4	—	52.17
Totals	20,795				1,225.91
<i>C. 1977 Chinese-owned firms in the San Francisco-Oakland SMSA</i>					
Construction	133	3	39	2.5	8.07
Manufacturing	225	5	71	12.5	21.56
Transportation & public utilities	72	2	13	5	2.34
Wholesale	164	4	32	5	47.28
Retail	1,688	39	41	5	245.29
Finance, insurance, & real estate	426	10	12	5	15.11
Service	1,568	36	21	4.5	60.88
Other	106	2	12	—	3.06
Totals	4,382				403.59
<i>D. 1982 totals for Chinese-owned firms</i>	9,182		28	5	872.94

Source: 1977 and 1982 Survey of Minority-Owned Business Enterprises. U.S. Department of Commerce. Bureau of the Census, Washington, DC, Government Printing Office, 1980 and 1986.

and restaurants. What is distinctive about the Cuban enclave is the larger concentration in construction and light industries such as furniture-making and cigar production. Light industries were common in the Chinese enclave during the nineteenth century (Chinn, Lai, and Choy 1969).

Both the Cuban and Chinese enclave economies have grown considerably since 1977. The expansion of the Chinese enclave has been fueled largely by new immigration and capital flows from Hong Kong and Taiwan (Chen 1980). Growth in the Cuban enclave has to a greater extent been generated through internal capital formation (Boswell and Curtis 1984; Portes and Bach 1985) and small business loans from the federal government. Between 1977 and 1982, the number of Cuban-owned businesses in the Miami SMSA almost tripled. Most of this

expansion was in very small businesses; per-firm receipts fell more than 50 percent in real dollars during the five-year period (inflation was approximately 50 percent). It is also possible that the growth of Cuban-owned businesses has so intensified intraenclave competition in sectors such as construction and wholesaling that numerous older firms have been losing their previous market-shares. Even in the retail service sectors, where receipt-volumes have expanded rapidly, the real value of per-firm receipts has declined.

Unfortunately, the 1982 Survey of Minority-Owned Businesses often fails to distinguish between different Asian groups. Consequently, comparable data on Chinese-owned firms in the San Francisco-Oakland SMSA in 1977 and 1982 are severely restricted. We can see that the number of Chinese-owned businesses expanded

quickly. However, the pace of growth was slower than that of Cuban-owned businesses in the Miami SMSA. Further, per-firm receipts declined (in real dollars) among Chinese-owned businesses in the San Francisco-Oakland SMSA, but to a lesser extent than was true of Cuban-owned firms in the Miami SMSA.

### Model

Our model considers the earnings of male<sup>2</sup> immigrants, aged 25 to 64, to be a function of their labor market experience, marital status, education, time devoted to work, English-language skills, citizenship, place of residence, time since immigration, and occupation. Immigrants who are self-employed are distinguished from immigrant-workers employed in the private sector.<sup>3</sup> Also, immigrants who live in an area of ethnic group concentration are distinguished from those who live in other areas. At times in the analysis, we consider geographically proximate non-Hispanic white immigrants in order to provide a baseline of comparison.<sup>4</sup> For Florida, we have six subsamples. After distinguishing employees in the private sector from the self-employed, we differentiate: (1) Cuban immigrants living in Miami and Hialeah; (2) Cuban immigrants living elsewhere in Florida; and (3) a reference group of non-Hispanic white immigrants living in Florida, but outside of Miami and Hialeah. Similarly, for California, we first distinguish employees in the

private sector from the self-employed, and then separate: (1) Chinese immigrants living in San Francisco; (2) Chinese immigrants living elsewhere in California; and (3) a reference group of non-Hispanic white immigrants living in California, but outside of San Francisco.<sup>5</sup>

To be sure, there is a considerable degree of self-selection involved in who resides where. For example, the Cuban enclave offers Cuban immigrants with poor English-language skills a place where they can more easily communicate with others and feel at ease. If these types of self-selection processes are important predictors of earnings, and if their influences are not adequately taken into account by the variables we control for, such as English skills, U.S. citizenship, and time of immigration, the model we examine is misspecified and our findings may be misleading. One variable in particular may be problematic. The place of residence of an immigrant is likely to be influenced by the geographical location of relatives. A potential difficulty arises because kinship ties may affect earnings *net* of place of residence and the other variables considered in our model. It is possible that immigrants who reside near relatives may systematically receive a boost in earnings because of assistance received from family members (or they may systematically experience reduced earnings due to family obligations). However, it is conceivable that once place of residence and human-capital characteristics such as English-language skills and education are controlled for, the hypothetical *direct* relationship between kinship ties and earnings is effectively mediated. If this is the case, the parameter estimates obtained from the equations we report are not biased by our inability to specify kinship ties.

The issue of self-selection can be conceived of as a problem of sample selection bias insofar as place of residence is partially determined by a process in which people are distributed across geographical areas in a nonrandom way. If one has an adequate theory of the selection process and available data to estimate the unknown parameters of that theory, methods suggested by Heckman (1976, 1979) may be used to reduce the gravity of biasness in the parameter estimates of the substantive model that is likely to exist *if* the substantive model is indeed misspecified (e.g., a selection process is operating, but the substantive model does not take into account how this process affects the dependent

<sup>2</sup> In order to be comparable to Portes et al., our samples are restricted to men. We are moving toward the study of families rather than individuals, but such an analysis is beyond the scope of this paper.

<sup>3</sup> Self-employment is defined as code (5) "self-employed worker—business not incorporated" or code (6) "Employee of our own corporation" of the variable CLASS in the PUMS file. Private sector employment is defined as code (1) "Private wage and salary worker: Employee of private company" of CLASS. For the purposes of our study, workers in the public sector (local, state, and federal government employees) are not considered.

<sup>4</sup> Prior to the Mariel exodus in 1980, the vast majority of displaced Cubans living in the U.S. were white. The 1980 Census data do *not* include participants of the Mariel flotilla. In the samples we analyze, none of the Cuban entrepreneurs and only 1.3 percent of the Cuban workers in Miami and Hialeah are black. One-fifth of one percent of the Cuban entrepreneurs and seven-tenths of one percent of the Cuban workers who reside elsewhere in Florida are black. We consider non-Hispanic white immigrants as a baseline comparative group to avoid attributing the costs of immigration (that is, the socioeconomic dislocations typically associated with immigration) to ethnicity or segregation. It is impractical to draw comparisons to non-Hispanic white immigrants residing in the enclave areas because of the limited number of such cases.

<sup>5</sup> In California and Florida, most of the non-Hispanic white immigrants in our samples hail from Canada and western European countries, especially the U.K., West Germany, and Italy. During the 1970s, fairly large numbers of non-Hispanic white immigrants in California also came from the U.S.S.R., Israel, Lebanon, and Iran.

variable). Unfortunately, we are unaware of a realistic selection theory that can be operationalized with the data at our disposal. We must rely on the adequacy of the specification of our substantive model.

The structural equation we analyze takes the following form:  $\ln(\text{EARNINGS}) = B_0 + B_1 \text{MKT-EXP} + B_2 \text{EXP-SQ} + B_3 \text{MARRIED} + B_4 \text{ELE-EDUC} + B_5 \text{HS-EDUC} + B_6 \text{COL-EDUC} + B_7 \ln(\text{HOURS WORKED}) + B_8 \text{ENGLISH SKILLS} + B_9 \text{U.S. CITIZENSHIP} + B_{10} +$

$\text{ETHNIC} + \sum_{i=1}^T B_i (\text{dummy variables differentiating time of immigration}) + \sum_{j=1}^J B_j (\text{dummy variables differentiating occupations}) + E.^6$

The natural logarithm of the sum of wages and salaries in 1979 serves as the measure of EARNINGS. For the self-employed, earnings are the sum of the variables INCOME1, INCOME2, and INCOME3 on the 1980 PUMS Census tapes. The former measure reflects wages and salaries (some self-employed immigrants in the samples are "employees" of their own firm); the latter two measures are reports of self-employment income.<sup>7</sup> Among employees in the private sector, the variable EARNINGS is simply INCOME1. Labor market experience is modeled as a quadratic function. MKP-EXP is computed as (years of age - [years of schooling - 4]) with a minimum value of one year. EXP-SQ is MKP-EXP squared. Inclusion of both variables in the equation is necessary to model the theoretical nonlinear relationship. MARRIED is a dummy variable coded 1 for immigrant men who are married and not separated from their spouses. Education is operationalized as a spline function. While the human-capital earnings function is usually

specified with one measure of education (years of schooling), we suggest that a year of additional schooling at different levels (e.g., high school versus college) may vary so much in its influence on earnings that the relationship between earnings and schooling is more appropriately modeled in a way that considers the level of schooling completed, as well as years of schooling *per se*. Thus, ELE-EDUC is measured as years of schooling up to and including the eighth grade (0 = no schooling; 1 = nursery school; 2 = kindergarten; 3 = first grade; . . . ; 10 = eighth grade). Everyone who attended the eighth grade or higher is coded 10 on ELE-EDUC. HS-EDUC is measured as years of schooling between the ninth (coded as 11) and twelfth (coded as 14) grades minus 10. All those who did not advance to the ninth grade are coded 0 on HS-EDUC and those who went on to college are coded 4 on HS-EDUC. COL-EDUC is coded as years of schooling between the first year of college (coded as 15) and the eighth year or more (coded as 22) of college minus 14. People who did not attend college are coded 0 on COL-EDUC. The natural logarithm of the approximate number of HOURS WORKED (number of weeks worked  $\times$  number of hours worked per week (maximum hours = 80)) in 1979 is controlled for here in place of the conventional human-capital earnings function formulation that considers only the logarithm of weeks worked. This point of departure in specification is due to the considerable variation in the number of hours worked weekly. In effect, we are attempting to capture the nonstochastic processes through which hourly wage rates are determined (Petersen 1985a). ENGLISH SKILLS is a Likert measure in which 0 represents proficiency in English and 4 represents poor English skills. U.S. CITIZENSHIP is a dummy variable coded 1 for immigrants who have obtained U.S. citizenship. ETHNIC is the percentage of Chinese (California) or Cuban (Florida) residents in cities, towns, and counties other than Miami-Hialeah and San Francisco. The variable is intended to control for the extent to which ethnic labor markets (see Mar 1984) operate outside of these cities.<sup>8</sup> This type of variable is termed a "global variable" (see Lazarsfeld and Menzel 1969) and is perhaps the only kind of contextual variable generally viewed as being theoretically informative in micro-level analy-

<sup>6</sup> The theoretical arguments behind the specification of the standard variables in the human capital-earnings function may be obtained from numerous sources (e.g., Chiswick 1978a; Mincer, 1974).

<sup>7</sup> It is widely recognized that underreports of earnings are common among the self-employed. If underreporting is consistent within ethnic groups (e.g., well educated Cuban entrepreneurs underreport their dollar-earnings as much (percentage-wise) as poorly educated Cuban entrepreneurs), the parameter estimates (including the intercept) and the *t*-statistics will be unaffected. If underreporting varies between ethnic groups (e.g., non-Hispanic white entrepreneurs underreport to a greater extent than Cuban entrepreneurs), the parameter estimates and the *t*-statistics will still be unaffected, as long as within-group underreporting is done consistently. Unfortunately, we do not know the extent to which underreports of earnings are consistent within the groups under examination.

<sup>8</sup> Using U.S. Department of Commerce (1983b), we measured ETHNIC for 53 locales in Florida and 52 locales in California. Some of the most lightly populated areas had a population of just over 2,500 residents in 1980.

ses.<sup>9</sup> The dummy variables that distinguish time of immigration (T) and occupations (J)<sup>10</sup> are self-explained in the tables. We reference time-since-immigration to the year in which a revolutionary government came to power: 1959 for Cuban immigrants and 1949 for Chinese immigrants.

In order to de-emphasize immigrants who might be viewed as only occasional workers, the samples we examine are restricted to men who worked the equivalent of four weeks on a full-time basis (160 hours) and who earned at least \$500. This means that our analyses are conducted on truncated samples. Though the proportion of cases omitted due to the minimum requirements of work-time and earnings is small (the range is from 1.25 to 4.37 percent in the 12 subsamples analyzed), in principle, it is still possible that the accuracy of our results is compromised (see Berk 1983). In this case, we believe that we can specify and estimate a model that effectively allows us to integrate the selection process into our substantive model. In empirical terms, we need to estimate an equation that provides a good prediction of which immigrants earned at least \$500 and worked at last 160 hours in 1979. The dependent variable of the selection model is a dummy variable coded 0 for those who failed to work or earn these required minimums. The exogenous variables include a dummy variable indicating whether a language other than English is spoken in the home; a dummy variable that indicates whether respondents have attended at least four years of high school; age; the approximate number of years that immigrants have resided in the U.S. (possible values include 2, 7, 12, 17, 25, and 35—greater accuracy is not possible using the PUMS data);

the interaction of age and years since immigration; and a dummy variable that distinguishes managers, administrators, executives, professionals, and technicians from other members of the labor force.

The selection equation accounts for more than one-third of the variance in the dichotomous dependent variable in each of the 12 subsamples. The procedure we follow is to construct a hazard rate using the predicted  $Y_s$  obtained from a probit analysis of the selection equation (see Heckman 1976, 1979; Berk 1983). The hazard rate is then added as an exogenous variable to the substantive model. Because very few parameter estimates or tests of statistical significance differ between specifications of the substantive model with and without the hazard rate included, we report results obtained from the more parsimonious specifications (i.e., equations estimated without the hazard rate). However, all estimates that differ in statistical significance across the two specifications are distinguished in the tables that follow. In each of these cases, both of the parameter estimates (and their  $t$ -ratios) are reported. The direct effect of the hazard rate is statistically significant in only 1 of the 12 substantive equations; this too is reported in the appropriate table.

### Findings

Table 2 provides descriptive data on the three subsamples of employees in Florida. The most striking observation is the disadvantaged character of Cuban immigrants in Miami and Hialeah compared to Cuban immigrants living elsewhere in Florida. Further, Cuban and non-Hispanic white immigrants living outside of Miami and Hialeah are quite similar. On average, Cubans in the enclave areas earned only about 70 percent as much in wages and salaries as the other groups. Several of the other differences shown in Table 2 may account for the disadvantage in earnings. For example, college education is substantially less common among Cubans in Miami and Hialeah, English-language skills are poorest among this group, and U.S. citizenship is less common. Combined, these variables suggest that enclave dwellers are culturally less assimilated than Cubans who live outside of Miami and Hialeah. This impression is supported by the fact that more recent cohorts of Cuban immigrants tend to be overrepresented in Miami and Hialeah. The occupational distributions of the two Cuban subsamples are more differentiated, as indicated by the index of dissimilarity, than are the occupational distributions of the Cuban and non-Hispanic white subsamples living outside of Miami and Hialeah.

In Table 3, the three-group comparison is

<sup>9</sup> See Blalock (1984) for a review of important issues pertaining to contextual analysis.

<sup>10</sup> Our model takes into account 11 occupational categories for employees and 6 occupational categories among the self-employed. In the latter case, some categories have been pooled due to an infrequency of cases. Occupations of the self-employed tend to cluster more than employee-occupations. Consequently, the occupational categories of the self-employed and private sector employees are not always comparable. We do not specify a segmented labor market. The extent to which theoretical sectors (e.g., primary and secondary) partition the entire labor market in sufficiently distinctive forms that they can be accurately specified in structural equations is unclear (Cain 1976; Smith 1983). Portes et al. define the enclave-labor market by firm ownership and geographic location. When we analyze the self-employed, we are considering both types of information. When we analyze private-sector employees, we consider only geographic location. Our data do not contain the ethnicity of employers for those sample members who work for others.



Table 2. Immigrant Employees in the Private Sector of Florida Who Worked at Least 160 Hours and Earned a Minimum of \$500 in 1979

	Cubans in Miami and Hialeah		Cubans Elsewhere in Florida		Whites (Non-Hispanic) <sup>e</sup>	
Totals	1,831		1,749		1,646	
Mean 1979 earnings <sup>a</sup>	11,734.306	(7,652.241) <sup>d</sup>	16,473.668	(11,488.853)	16,407.312	(11,862.391)
Mean 1979 log-earnings	9.164	(.699)	9.489	(.713)	9.439	(.804)
Mean labor-market experience	30.291	(12.062)	25.355	(12.188)	25.185	(12.682)
Mean labor-market experience squared	1,062.969	(693.975)	791.338	(634.654)	794.993	(665.711)
% married	.799		.844		.772	
Mean elementary education	9.191	(1.608)	9.621	(1.118)	9.816	(1.004)
Mean high school education	2.222	(1.904)	2.946	(1.693)	3.369	(1.328)
% four years of high school <sup>a</sup>	.511		.703		.789	
Mean college education	.974	(1.997)	1.652	(2.261)	1.554	(2.116)
% four years of college <sup>a</sup>	.124		.234		.215	
Mean hours worked in 1979 <sup>a</sup>	1,956.134	(619.936)	2,084.168	(570.884)	2,049.465	(676.781)
Mean log-hours worked in 1979	7.494	(.490)	7.583	(.404)	7.540	(.486)
Mean English-language skills <sup>b</sup>	2.543	(1.043)	1.941	(1.016)	.655	(.843)
% U.S. citizens	.394		.580		.592	
% ethnic	.491 <sup>a</sup>		.188		.040 <sup>a</sup>	
% immigrating 1975-1979	.057		.031		.180	
% immigrating 1970-1974	.223		.126		.108	
% immigrating 1965-1969	.332		.256		.108	
% immigrating 1960-1964	.289		.429		.134	
% immigrating prior to 1960	.099		.158		.470	
Executive, administrative, & managerial (3-37) <sup>c</sup>	.100		.172		.174	
Professional specialties (43-199)	.036		.074		.087	
Technicians (203-235)	.023		.034		.035	
Sales (243-285)	.078		.125		.123	
Administrative support including clerical (303-389)	.087		.078		.057	
Business, protective, & household services (403-469)	.119		.079		.109	
Farming, forestry, & fishing (473-499)	.010		.019		.016	
Precision production & craft (503-699)	.246		.232		.269	
Operators (703-799)	.128		.084		.047	
Transportation (803-859)	.082		.056		.043	
Laborers (863-919)	.091		.047		.039	
Index of dissimilarity in occupations:	.177		.084			

<sup>a</sup> Variable not included in least-squares analyses.<sup>b</sup> Likert scale (0-4). Large numbers indicate poor English-language skills.<sup>c</sup> Numbers in parentheses are 1980 standard occupational classification codes.<sup>d</sup> Standard deviations are in parentheses.<sup>e</sup> Outside of Miami and Hialeah.

much the same. Self-employed Cuban immigrants in Miami and Hialeah earn only about 70 percent as much as self-employed Cuban immigrants living in Florida, but outside of

Miami and Hialeah. This latter Cuban group closely parallels the non-Hispanic white immigrants in terms of earnings and human-capital characteristics. In contrast, Cubans in Miami

Table 3. Immigrant *Entrepreneurs in Florida* Who Worked at Least 160 Hours and Earned a Minimum of \$500 in 1979

	Cubans in Miami and Hialeah		Cubans Elsewhere in Florida		Whites (Non-Hispanic) <sup>e</sup>	
Totals	418		490		609	
Mean 1979 earnings <sup>a</sup>	16,396.543	(16,060.314) <sup>d</sup>	24,134.776	(21,691.471)	23,590.222	(22,942.951)
Mean 1979 log-earnings	9.349	(.869)	9.713	(.917)	9.659	(.956)
Mean labor-market experience	30.892	(10.706)	26.208	(10.744)	26.236	(11.931)
Mean labor-market experience squared	1,068.691	(651.630)	802.057	(577.369)	830.456	(655.427)
% married	.900		.939		.854	
Mean elementary education	9.232	(1.566)	9.557	(1.426)	9.788	(.996)
Mean high school education	2.282	(1.874)	3.059	(1.606)	3.266	(1.419)
% four years of high school <sup>a</sup>	.512		.722		.759	
Mean college education	1.244	(2.308)	2.380	(2.886)	2.103	(2.710)
% four years of college <sup>a</sup>	.148		.339		.292	
Mean hours worked in 1979 <sup>a</sup>	2,127.038	(805.803)	2,282.784	(765.043)	2,229.043	(814.967)
Mean log-hours worked in 1979	7.559	(.524)	7.644	(.497)	7.607	(.529)
Mean English-language skills <sup>b</sup>	2.433	(.997)	1.839	(.936)	.718	(.827)
% U.S. citizens	.483		.649		.622	
% ethnic	.490 <sup>a</sup>		.201		.036 <sup>a</sup>	
% immigrating 1975-1979	.024		.016		.154	
% immigrating 1970-1974	.213		.096		.128	
% immigrating 1965-1969	.294		.212		.115	
% immigrating 1960-1964	.340		.482		.118	
% immigrating prior to 1960	.129		.194		.485	
Executive, administrative, & managerial (3-37) <sup>c</sup>	.220		.229		.304	
Professional specialties & technicians (43-235)	.117		.175		.151	
Sales (243-285)	.172		.173		.169	
Business, protective, & household services (403-469)	.036		.063		.061	
Precision production & craft (503-699)	.273		.227		.218	
Administrative support including clerical; farming, forestry, & fishing; operators; transportation; laborers (303-389, 473-499, 703-919)	.182		.133		.097	
Index of dissimilarity in occupations:	.097		.075			

<sup>a</sup> Variable not included in least-squares analyses.<sup>b</sup> Likert scale (0-4). Large numbers indicate poor English-language skills.<sup>c</sup> Numbers in parentheses are 1980 standard occupational classification codes.<sup>d</sup> Standard deviations are in parentheses.<sup>e</sup> Outside of Miami and Hialeah.

and Hialeah tend to be less educated, have poorer English-language skills, and are less likely to be citizens of the U.S. A comparison across Tables 2 and 3 reveals that each of the three subgroups of private-sector employees

typically earns only about 70 percent as much as its self-employed counterparts. Comparing the self-employed to the private-sector employees, whether they are Cuban immigrants in Miami and Hialeah, Cuban immigrants living else-

where in Florida, or non-Hispanic white immigrants, self-employed workers are generally better educated and are more likely to be citizens of the U.S. The overall picture obtained from Tables 2 and 3 is that self-employed immigrants have a number of advantages over immigrants employed in the private sector. Further, Cuban immigrants in Miami and Hialeah, both the self-employed and employees in the private sector, are typically disadvantaged when compared to Cuban immigrants outside of Miami and Hialeah.

To what extent are the descriptive inequalities in earnings a function of unequal earning-returns to human capital? By estimating our modified model of the human-capital earnings function as an analysis of covariance and then estimating the model a second time, pooling the groups that are distinguished in the former analysis, we are able to test whether various subsamples tend to receive distinctive returns to human-capital characteristics. The results of these tests are reported in Table 4.

First we asked whether the strategy employed by Portes et al. of pooling private-sector employees and self-employed Cuban immigrants is appropriate. The results in Table 4 demonstrate that such a strategy is unwarranted, given our data and model. Equation 1 in Table 4

indicates that, among Cuban immigrants in Miami and Hialeah, private-sector employees and the self-employed should be treated as members of different populations in terms of the earning-returns generally obtained for human capital. With this information, the next logical step is to test whether it is appropriate for us to pool Cuban immigrants from Miami and Hialeah. For both the self-employed (equation 2) and private-sector employees (equation 3), it is apparent that our implied hypothesis that it is proper to pool Cuban immigrants from Miami and Hialeah is supported by the data. Equation 4 indicates that private-sector employees and self-employed Cuban immigrants living outside of Miami and Hialeah should be distinguished. This finding is analogous to the results reported in equation 1 and, together, the tests indicate that we should analyze the earning-returns to human-capital characteristics of Cuban immigrants who are self-employed separately from the Cuban immigrants who are employed in the private sector.

The second question we raised is whether Cuban immigrants in the ethnic enclave typically receive earning-returns to human-capital characteristics on a par with Cuban immigrants living elsewhere in Florida. In Table 4 our results indicate that, for both the self-employed (equation 5) and private-sector employees (equation 6), Cuban immigrants in Miami and Hialeah tend to receive different returns to human capital than do their counterparts outside of Miami and Hialeah. If these differences tend to favor immigrants in Miami and Hialeah, the findings would provide support for the enclave-economy hypothesis. However, the estimates do *not* indicate this. Cuban immigrants in Miami and Hialeah who are employed in the private sector typically receive lower returns to human-capital characteristics than Cuban immigrants employed in the private sector who live elsewhere in Florida. This is contrary to the arguments of the enclave-economy hypothesis. Once a distinction is drawn between relatively advantaged self-employed immigrants and immigrants employed in the private sector, and once an additional distinction between residence in enclave and non-enclave areas is made, there is no evidence in support of the enclave-economy hypothesis in respect to employees in the private sector. On the other hand, the differences in earning-returns acquired by self-employed Cuban immigrants vary just slightly across geographical locales. Hence, despite the significant *F*-test, our findings are generally consistent with the enclave-economy hypothesis when only self-employed Cuban immigrants are considered.

One reason Cuban immigrants employed in the private sector tend to receive poor earning-returns to human capital might be that most of

Table 4. Comparative *F*-tests Among Cuban Immigrants

Equation	Subgroups Compared (Underscored)	<i>F</i> -ratio and Degrees of Freedom
1	A comparison of <u>self-employed</u> and <u>private-sector employees</u> in <u>Miami</u> and <u>Hialeah</u>	$F_{2,211}^{19} = 2.15^*$
2	<u>Self-employed</u> in <u>Miami</u> compared to self-employed in <u>Hialeah</u>	$F_{380}^{19} = 1.59$
3	<u>Private-sector employees</u> in <u>Miami</u> compared to private-sector employees in <u>Hialeah</u>	$F_{1,783}^{24} = 1.37$
4	A comparison of <u>self-employed</u> and <u>private-sector employees</u> outside of <u>Miami</u> and <u>Hialeah</u>	$F_{2,199}^{20} = 2.05^*$
5	<u>Self-employed</u> in <u>Miami</u> and <u>Hialeah</u> compared to self-employed outside of <u>Miami</u> and <u>Hialeah</u>	$F_{869}^{19} = 1.68^*$
6	<u>Private-sector employees</u> in <u>Miami</u> and <u>Hialeah</u> compared to private-sector employees outside of <u>Miami</u> and <u>Hialeah</u>	$F_{3,531}^{24} = 3.88^*$

\*  $p \leq .05$ .

the workers are in the secondary labor market. However, using the labor market definitions employed by Portes et al. (these definitions change from time to time in the literature, e.g., Wilson and Portes 1980; Portes and Bach 1985) we would expect only about one-third of the Cuban immigrants in Miami and Hialeah (the same communities in which three-quarters of the Cuban immigrants in the Portes et al. sample reside; see Portes and Bach 1985, p. 171, Table 46) who are employed in the private sector to be in the secondary labor market. This estimate is based on information provided by Portes and Bach (1985, p. 216-18). Moreover, unlike the sample analyzed by Portes et al., many of the Cuban immigrants in the Census sample came to the U.S. at an early age and therefore are more likely to have the benefit of educational and socialization experiences in this country that enhance the chances of avoiding employment in the secondary labor market. Consequently, it seems unlikely that the Census sample of Cuban immigrants in Miami and Hialeah is predominantly composed of employees in the secondary labor market. Indeed, the occupational distribution of Cuban immigrants in Miami and Hialeah who are employed in the private sector (see Table 2) reveals that approximately 40 percent hold executive, managerial, professional, technical, and craft-related jobs.

Portes (personal correspondence) questions whether the comparatively low earning-returns we obtain for Cuban workers in the enclave are due to our concentration on Miami and Hialeah. He suggested that we might focus on all of Dade County and avoid the possibility of overrepresenting the least successful employees of the enclave if a large and comparatively successful contingent of enclave-employees resides elsewhere in the metropolitan area. Consequently, we estimated the model using Dade County to represent the limits of the enclave. Because the two sets of results are so similar, we report those estimated from the model that adheres to our preferred spatial representation of the enclave (i.e., Miami and Hialeah). However, at the appropriate places, we report the differences that do exist between the alternative analyses.<sup>11</sup>

<sup>11</sup> It may be preferable to operationalize enclave participants in terms of place of work rather than place of residence. Unfortunately, a few variables, including place of work, were coded by the Census Bureau for only a subsample of all cases in an effort to cut costs. Attrition in our samples is greater than 50 percent. It is clear that the selection strategy for coding place of work was carried out in a nonrandom way. For example, see pages 35 and K32-K33 in ICPSR's codebook for the 1980 Census of Population and Housing (U.S. Department of Commerce 1983a). Cuban employees who are well educated and have high earnings are overrepresented in the subsample for which place of work is coded.

We now consider these analyses in detail. In Table 5, the parameter estimates consistently indicate that Cuban immigrants who are employed in the private sector and reside in Miami and Hialeah receive comparatively low earnings in return for their human-capital characteristics. This is contrary to the enclave-economy hypothesis. Several relationships differ substantially between the two subgroups of Cuban immigrants (equations 1 and 2). For example, the plateau in earnings relative to years of labor market experience occurs somewhat earlier among Cubans in the enclave than among Cubans outside of Miami and Hialeah.<sup>12</sup> Probably more important, however, is that Cuban immigrants in Miami and Hialeah appear to receive no return to education, while Cuban immigrants who reside outside of Miami and Hialeah appear to enjoy an average increase in earnings of 5.4 percent for each additional year of schooling at the college level.<sup>13</sup> Based on the average earnings of Cuban immigrants employed in the private sector who live outside of Miami and Hialeah (see Table 3), this estimate reflects a \$890 return for each year of college education. The approximate average value in

Alternatively, employees who are recent immigrants are underrepresented. Among Cuban entrepreneurs, those with the highest earnings are overrepresented. These sampling biases are statistically significant (bivariate *t*-tests) at the .05 level for a two-tail test.

We re-examined our model considering only the subsamples for which enclave affiliation could be defined in terms of place of work within the Miami SMSA. Due to sampling biases, the chance of obtaining results consistent with the enclave-economy hypothesis is increased. Yet, the results of this analysis also reveal that Cuban enclave-workers are disadvantaged in returns to human capital compared to Cuban workers outside the enclave ( $F = 1.67$ , 24 and 1,476 d.f.,  $p < .05$ ). To the contrary, Cuban immigrant entrepreneurs in the enclave receive returns to their human capital that are comparable to the returns of Cuban immigrant entrepreneurs outside the enclave ( $F = 1.45$ , 19 and 343 d.f.,  $p > .05$ ). Hence, whether the enclave is defined in terms of place of residence or place of work, our results are consistent with the enclave-economy hypothesis in the case of entrepreneurs, but inconsistent with the hypothesis with respect to employees.

<sup>12</sup> The "plateau" of this curve is obtained by dividing the parameter estimate of labor-market experience by twice the parameter estimate of labor-market experience squared (see note f in Table 5).

<sup>13</sup> If Dade County is defined as the enclave, the magnitude of the disadvantage in returns to schooling among enclave-workers increases slightly despite the existence of a modest return for enclave-workers. Cuban workers in the enclave typically receive a three percent return to years of schooling at the college level, while Cuban workers elsewhere in Florida receive a nine percent return. This finding is virtually identical to that obtained from the analysis of the subsample for which place of work is used to define the enclave.

Table 5. Immigrant Employees in the Private Sector in Florida Who Worked at Least 160 Hours and Earned a Minimum of \$500 in 1979

Dependent Variable: 1979 Earnings (ln)	Cubans in Miami and Hialeah		Cubans Elsewhere in Florida		Whites (Non-Hispanic)*	
Equations	B <sup>a</sup>	(1) t-ratio <sup>b</sup>	B	(2) t-ratio	B	(3) t-ratio
Intercept	4.883	19.785	4.131	13.672	3.721	11.259
Labor-market experience	.009	1.731(24.8) <sup>c</sup>	.013	2.585(28.6)	.026	4.401(30.7)
Labor-market experience squared	-.018	-2.035 <sup>a</sup>	-.022	-2.357 <sup>a</sup>	-.043	-3.763 <sup>e</sup>
Married	.151	4.226	.201	5.096	.213	5.290
Elementary education	-.002	-.187	.019	1.216	-.019	-1.002
High school education	-.005	-.452	.003	.224	.013	.810
College education	.010	1.258	.054	7.048	.053	5.698
Hours worked (ln)	.591	20.582	.654	18.912	.739	21.241
English skills	-.031	-1.657	-.031	-1.609	-.059	-2.847
U.S. citizenship	.066	1.993	.140	4.389	-.061	-1.434
Ethnic		<sup>d</sup>	.000	.185	<sup>d</sup>	<sup>d</sup>
Immigrants 1975-1979	-.485	-6.361	-.236	-2.653	-.156	-2.672
Immigrants 1970-1974	-.058	-1.034	-.043	-.771	-.140	-2.289 <sup>f</sup>
Immigrants 1965-1969	-.093	-1.787	-.073	-1.546	-.004	-.069
Immigrants 1960-1964	.010	.195	.021	.508	.033	.626
Professional specialties	-.058	-.687	-.102	-1.654	-.045	-.656
Technicians	-.119	-1.190	-.253	-3.116	-.153	-1.587
Sales	-.090	-1.382	-.091	-1.763	-.122	-2.001
Administrative support incl. clerical	-.261	-4.113	-.312	-5.221	-.244	-3.083
Business, protective, & household	-.463	-7.581	-.502	-8.047	-.405	-6.039
Farming, forestry, & fishing	-.419	-2.928	-.483	-4.451	-.437	-3.247
Precision production & craft	-.122	-2.258	-.228	-4.808	-.149	-2.815
Operators	-.224	-3.644	-.247	-3.957	-.263	-3.039
Transportation	-.299	-4.468	-.122	-1.751	-.228	-2.554
Laborers	-.324	-4.914	-.463	-6.099	-.357	-3.828
R <sup>2</sup>		.318		.363		.340

<sup>a</sup> Unstandardized regression coefficients.<sup>b</sup> A t-ratio  $\geq 1.96$  indicates that  $p \leq .05$ , two-tail test. A t-ratio  $\geq 1.65$  indicates that  $p \leq .05$ , one-tail test.<sup>c</sup> Years of labor-market experience at which earnings reach a plateau.<sup>d</sup> Variable is omitted. In equation 1, Ethnic has no variance. In equation 3, Ethnic is unwarranted on theoretical grounds.<sup>e</sup> The decimal point of the parameter estimate is moved 2 places to the right.<sup>f</sup> When the selection hazard ratio is controlled for  $B = -.121$  and the t-ratio = -1.797.<sup>g</sup> Outside of Miami and Hialeah.

annual earnings attributable to holding a college degree would then be in the neighborhood of \$3,560. Further, the relative advantage of being a U.S. citizen is roughly twice as great among Cubans outside of the enclave (14 percent) as for Cubans in the enclave (6.6 percent),<sup>14</sup> and the net cost of being a recent (1975-1979) rather than an older (prior to 1960) immigrant is roughly twice as great among Cubans in Miami and Hialeah (48.5 percent) as among Cubans living elsewhere in Florida (23.6 percent). In contrast, the parameter estimates pertaining to

Cuban immigrants (equation 2) and non-Hispanic white immigrants (equation 3) living outside of Miami and Hialeah are typically similar. However, U.S. citizenship seems to provide Cuban immigrants with an advantage in earnings that is not apparent among the non-Hispanic white immigrants. Further, English language skills appear to enhance the earnings of only non-Hispanic white immigrants. Our analysis of Cuban immigrant employees in the private sector indicates that, while Cubans in the outside economy hold their own very well in comparison to non-Hispanic white immigrants,<sup>15</sup> Cubans in Miami and

<sup>14</sup> This finding does not replicate if Dade County is defined as the enclave. Cuban workers, both in and out of the enclave, tend to receive equal returns (11 percent) to U.S. citizenship. However, the disadvantage among enclave workers does replicate when we consider only Cubans for whom place of work is coded.

<sup>15</sup> An F-test between equations 2 and 3 in Table 5 is significant ( $F = 1.65$ , 24 and 3,346 d.f.,  $p < .05$ ), but, as in the case of equation 5 of Table 4, inspection of the

Hialeah tend to be disadvantaged in earning-returns.<sup>16</sup>

A different story emerges from Table 6. Self-employed Cuban immigrants in Miami and Hialeah typically receive equal returns to human capital compared to either Cuban or non-Hispanic white immigrants in the outside economy. There are some differences; for instance, self-employed Cuban immigrants in Miami and Hialeah receive comparatively less return to labor-market experience, but they appear to benefit most from the acquisition of English-language skills. Overall, the differences in parameter estimates across equations 2 and 3 in Table 6 are minimal ( $F = .53$ , 19 and 1,060 d.f.,  $p > .05$ ). Self-employment in Miami and Hialeah appears to provide an avenue

through which immigrants participating in the Cuban enclave economy can achieve comparable returns to their human-capital characteristics.<sup>17</sup>

To this point, our examination suggests that the enclave-economy hypothesis has merit when limited to the self-employed, but is soundly rejected in the case of private-sector employees. We now attempt to discover whether these findings can be replicated and shown to be generalizable beyond the Cuban case.

Table 7 provides data on the three subsamples of immigrant private-sector employees in California. Chinese immigrants residing in San Francisco are disadvantaged in virtually every variable we measure. They have lower earnings, more modest levels of educational attainment, weaker English-language skills, and they are less likely to be citizens of the U.S. Chinese immigrants in San Francisco appear to be less culturally assimilated than Chinese immigrants who live elsewhere in California. But Chinese immigrants living outside of San Francisco are also disadvantaged in several respects compared to non-Hispanic white immigrants residing outside of San Francisco. Chinese immigrants are more recent arrivals than non-Hispanic white immigrants. Fifty-three percent of the Chinese immigrants have arrived since 1970, while only 26 percent of non-Hispanic white immigrants have arrived since 1970. Economic dislocations due to immigration are most severe in the early years of adaptation to U.S. society (Chiswick 1978, 1980). Chinese immigrants living outside of San Francisco are substantially more likely to have attended at least four years of college than non-Hispanic white immigrants. This advantage in education appears to be reflected in the comparatively large proportions of professional and white-collar workers among Chinese immigrants living outside of San Francisco. Despite these advantages, Chinese immigrants living outside of San Francisco average only 78 percent of the earnings of non-Hispanic white immigrants outside of San Francisco. Chinese in San Francisco averaged only 72 percent of the earnings of Chinese living elsewhere.

parameter estimates indicates that the two subsamples are more equal than different.

<sup>16</sup> A related issue should be addressed at this point. Portes et al. contend that there is an important lagged return to employment in the enclave such that the likelihood of eventually becoming self-employed is enhanced by serving as an enclave-worker. We do not have the data to test this hypothesis. Portes and Bach (1985, p. 214, Table 70) report that Cubans employed by Anglos in 1976 were 15 percent less likely to be self-employed in 1979 than Cubans who worked for non-Anglos (usually Cubans) in 1976. Although this estimate is incorrect, the correct metric probability (based on the mean of the dependent variable) is 11.4 percent (see Petersen 1985b); the relationship still suggests that employment in the enclave does facilitate an eventual move into self-employment. While the implications of this finding are exciting and, in our view, potentially constitute the most important information provided by Portes et al., we remain troubled by other data that seem to suggest the Dade County enclave may *not* contribute to unusually high rates of self-employment. Between 1977 and 1982, the growth rate of Cuban-owned firms in the Miami SMSA (183 percent) was greater than in some SMSAs (Ft. Lauderdale-Hollywood, 151 percent; Tampa-St. Petersburg, 125 percent), but lower than in other SMSAs (West Palm Beach-Boca Raton, 248 percent) that contain numerous Cuban businesses (U.S. Department of Commerce 1986, 1980). Further, using the five percent PUMS data to conduct a cross-county comparison of self-employment among male Cuban immigrants in Florida, we find that the rate of self-employment in Dade County is the same as the average rate of self-employment elsewhere in Florida (20.2 percent). The six counties where the greatest number of Cubans reside have the following percentages of self-employment among male immigrants from Cuba: Broward (19.6), Dade (20.2), Hillsborough (19.1), Monroe (21.0), Orange (33.3), and Palm Beach (15.5). Similarly, in the samples we analyze, 21.9 percent of the Cuban immigrants outside of Miami and Hialeah are self-employed, compared to an 18.6 percent rate of self-employment in Miami and Hialeah. The same holds in our California samples where 20.8 percent of the Chinese immigrants outside of San Francisco are self-employed compared to a 14.4 percent rate of self-employment in San Francisco.

<sup>17</sup> Strictly speaking, these findings do not represent empirical evidence in support of the enclave-economy hypothesis. About all that can be said is that our results are consistent with the enclave-economy hypothesis when it is restricted to only self-employed Cuban immigrants. Hodson and Kaufman (1982, p. 733) note this problem in works that use segmented structures in the economy as the null hypothesis and thereby put the burden of proof on evidence that would lead us to reject the existence of a segmented structure. The burden of proof must be placed on evidence that would lead us to accept a segmented structure before the claim can be made that the existence of a hypothetical segmented structure has been demonstrated through an empirical test.

Table 6. Immigrant *Entrepreneurs in Florida* Who Worked at Least 160 Hours and Earned a Minimum of \$500 in 1979

Dependent Variable: 1979 Earnings (ln)	Cubans in Miami and Hialeah		Cubans Elsewhere in Florida		Whites (Non-Hispanic) <sup>b</sup>	
Equations	B <sup>a</sup>	(1) t-ratio <sup>b</sup>	B	(2) t-ratio	B	(3) t-ratio
Intercept	5.777	8.214	5.148	7.559	5.457	8.247
Labor-market experience	.029	1.631(ns) <sup>c</sup>	.030	2.010(19.7) <sup>f</sup>	.040	2.844(28.6)
Labor-market experience squared	-.042	-1.414 <sup>e</sup>	-.076	-2.690 <sup>e, g</sup>	-.070	-2.690 <sup>e</sup>
Married	.176	1.325	.411	2.691	.253	2.477
Elementary education	.039	1.171	.014	.410	.021	.516
High school education	-.046	-1.444	.014	.440	.026	.778
College education	.073	3.267	.066	3.438	.083	4.680
Hours worked (ln)	.389	4.981	.500	6.743	.429	6.378
English skills	-.140	-2.739	-.006	-.116	-.020	-.433
U.S. citizenship	.122	1.224 <sup>d</sup>	.019	.232	.039 <sup>d</sup>	.402 <sup>d</sup>
Ethnic			.003	1.190		
Immigrants 1975-1979	-.058	-.202	.014	.046	.013	.098
Immigrants 1970-1974	.184	1.162	-.200	-1.283	-.193	-1.485
Immigrants 1965-1969	.065	.469	-.079	-.661	.037	.291
Immigrants 1960-1964	.153	1.150	.066	.673	-.022	-.184
Professional specialties; technicians	-.063	-.402	-.095	-.703	-.253	-1.977
Sales	-.254	-2.016	-.238	-2.085	-.219	-2.022
Business, protective, & household services	-.542	-2.395	-.451	-2.776	-.471	-2.961
Precision production & craft	-.147	-1.264	-.236	-2.108	-.260	-2.533
Administrative support incl. clerical; farming, forestry, & fishing; operators; transportation; laborers	-.256	-1.946	-.292	-2.257	-.452	-3.412
R <sup>2</sup>		.193		.303		.201

<sup>a</sup> Unstandardized regression coefficients.<sup>b</sup> A t-ratio  $\geq 1.96$  indicates that  $p \leq .05$ , two-tail test. A t-ratio  $\geq 1.65$  indicates that  $p \leq .05$ , one-tail test.<sup>c</sup> Years of labor-market experience at which earnings reach a plateau. (ns = non-significant).<sup>d</sup> Variable is omitted. In equation 1, Ethnic has no variance. In equation 3, Ethnic is unwarranted on theoretical grounds.<sup>e</sup> The decimal point of the parameter estimate is moved 2 places to the right.<sup>f</sup> When the selection hazard rate is controlled for  $B = .022$  and the t-ratio = 1.402.<sup>g</sup> When the selection hazard rate is controlled for,  $B = -.056$  and the t-ratio = -1.811.<sup>b</sup> Outside of Miami and Hialeah.

A similar picture emerges from Table 8. Self-employed Chinese immigrants in San Francisco appear to be disadvantaged in comparison to self-employed Chinese immigrants outside of San Francisco. This latter group appears to be disadvantaged compared to self-employed non-Hispanic white immigrants outside of San Francisco. Comparing across Tables 7 and 8, in each of the three subgroups, private-sector employees earn substantially less than their self-employed counterparts. Descriptively, the California samples have many similarities with the Florida samples. The major difference is that, while Cuban private sector employees and the self-employed Cuban immigrants living outside of Miami and Hialeah compared rather equally with non-Hispanic white immigrants in descriptive terms, Chinese private-sector employees and Chinese self-

employed immigrants living outside of San Francisco compare unfavorably in earnings with non-Hispanic white immigrants, despite having a substantial advantage in education.

Table 9 reports a series of F-tests for the Chinese samples. These tests help us to evaluate whether the various California subsamples receive different returns to their human-capital characteristics. Equation 1 indicates that private-sector employees and self-employed Chinese immigrants in San Francisco should be analyzed separately. Likewise, equation 2 indicates that separate analyses are called for when private sector employees and self-employed Chinese immigrants outside of San Francisco are examined. In contrast, equation 3 indicates that self-employed Chinese immigrants in California receive roughly the same returns to human-capital characteristics regardless of whether they

Table 7. Immigrant Employees in the Private Sector of California Who Worked at Least 160 Hours and Earned a Minimum of \$500 in 1979

	Chinese in San Francisco		Chinese Elsewhere in California		Whites (Non-Hispanic) <sup>a</sup>	
Totals	465		1,417		8,137	
Mean 1979 earnings <sup>a</sup>	11,688.817	(9,123.054) <sup>d</sup>	16,283.126	(10,875.518)	20,743.560	(12,935.722)
Mean 1979 log-earnings	9.103	(.788)	9.430	(.825)	9.711	(.771)
Mean labor-market experience	25.940	(13.537)	18.342	(12.826)	23.131	(12.360)
Mean labor-market experience squared	855.725	(762.179)	500.838	(629.524)	687.781	(622.368)
% married	.798		.776		.743	
Mean elementary education	8.899	(2.687)	9.517	(1.860)	9.761	(1.186)
Mean high school education	2.615	(1.781)	3.407	(1.358)	3.454	(1.272)
% four years of high school <sup>a</sup>	.600		.829		.823	
Mean college education	1.432	(2.112)	3.322	(2.872)	2.259	(2.490)
% four years of college <sup>a</sup>	.232		.523		.329	
Mean hours worked in 1979 <sup>a</sup>	1,862.187	(629.323)	1,934.752	(630.502)	2,014.731	(604.481)
Mean log-hours worked in 1979	7.439	(.500)	7.484	(.480)	7.534	(.457)
Mean English-language skills <sup>b</sup>	2.308	(.995)	1.813	(.928)	.775	(.940)
% U.S. citizens	.443		.463		.558	
% ethnic	.121 <sup>a</sup>		.016		.012 <sup>a</sup>	
% immigrating 1975-1979	.314		.303		.159	
% immigrating 1970-1974	.200		.228		.100	
% immigrating 1965-1969	.209		.212		.122	
% immigrating 1960-1964	.062		.091		.170	
% immigrating 1950-1959	.081		.093		.307	
% immigrating prior to 1950	.134		.073		.142	
Executive, administrative, & managerial (3-37) <sup>c</sup>	.071		.140		.173	
Professional specialties (43-199)	.065		.258		.163	
Technicians (203-235)	.039		.076		.048	
Sales (243-285)	.060		.074		.095	
Administrative support including clerical (303-389)	.103		.055		.059	
Business, protective, & household services (403-469)	.417		.210		.061	
Farming, forestry, & fishing (473-499)	.000 <sup>a</sup>		.005		.022	
Precision production & craft (503-699)	.138		.103		.242	
Operators (703-799)	.065		.047		.070	
Transportation (803-859)	.017		.011		.038	
Laborers (863-919)	.026		.020		.030	
Index of dissimilarity in occupations:	.319		.273			

<sup>a</sup> Variable not included in least-squares analyses.<sup>b</sup> Likert scale (0-4). Large numbers indicate poor English-language skills.<sup>c</sup> Numbers in parentheses are 1980 standard occupational classification codes.<sup>d</sup> Standard deviations are in parentheses.<sup>a</sup> Outside of San Francisco.



Table 8. Immigrant *Entrepreneurs in California* Who Worked at Least 160 Hours and Earned a Minimum of \$500 in 1979

	Chinese in San Francisco		Chinese Elsewhere in California		Whites (Non-Hispanic) <sup>e</sup>	
Totals	78		372		2,446	
Mean 1979 earnings <sup>a</sup>	14,036.795	(13,368.377) <sup>d</sup>	24,473.012	(22,208.659)	28,015.998	(23,425.019)
Mean 1979 log-earnings	9.238	(.804)	9.712	(.948)	9.870	(.947)
Mean labor-market experience	27.667	(13.565)	23.180	(12.400)	25.300	(11.252)
Mean labor-market experience squared	947.103	(787.714)	690.669	(680.019)	766.417	(591.009)
% married	.833		.909		.818	
Mean elementary education	8.974	(2.725)	9.478	(2.029)	9.820	(.945)
Mean high school education	2.615	(1.853)	3.360	(1.375)	3.425	(1.289)
% four years of high school <sup>a</sup>	.628		.804		.809	
Mean college education	1.436	(2.339)	3.132	(2.912)	2.364	(2.708)
% four years of college <sup>a</sup>	.179		.501		.339	
Mean hours worked in 1979 <sup>a</sup>	2,258.564	(729.941)	2,366.750	(820.921)	2,243.658	(804.426)
Mean log-hours worked in 1979	7.641	(.486)	7.680	(.489)	7.618	(.515)
Mean English-language skills <sup>b</sup>	2.115	(.939)	1.876	(.875)	.799	(.851)
% U.S. citizens	.628		.621		.613	
% ethnic	.121 <sup>a</sup>		.015		.012 <sup>a</sup>	
% immigrating 1975-1979	.192		.218		.129	
% immigrating 1970-1974	.128		.228		.090	
% immigrating 1965-1969	.244		.175		.115	
% immigrating 1960-1964	.064		.089		.160	
% immigrating 1950-1959	.103		.121		.311	
% immigrating prior to 1950	.268		.169		.195	
Executive, administrative, & managerial (3-37) <sup>c</sup>	.231		.358		.254	
Professional specialties & technicians (43-235)	.064		.191		.178	
Sales (243-285)	.167		.188		.187	
Business, protective, & household services (403-469)	.244		.121		.039	
Precision production & craft (503-699)	.205		.075		.233	
Administrative support including clerical; farming, forestry, & fishing; operators; transportation; laborers (303-389, 473-499, 703-919)	.089		.067		.109	
Index of dissimilarity in occupations:	.275		.200			

<sup>a</sup> Variable not included in least-squares analyses.<sup>b</sup> Likert scale (0-4). Large numbers indicate poor English-language skills.<sup>c</sup> Numbers in parentheses are 1980 standard occupational classification codes.<sup>d</sup> Standard deviations are in parentheses.<sup>e</sup> Outside of San Francisco.

live in San Francisco or elsewhere. Equation 4 suggests that Chinese private-sector employees in San Francisco should be analyzed separately from Chinese private-sector employees living

elsewhere in California. The results closely approximate the Cuban case. Only among self-employed immigrants do we find evidence consistent with the argument that participation

Table 9. Comparative *F*-tests Among Chinese Immigrants

Equation	Subgroups Compared (Underscored)	<i>F</i> -ratio and Degree of Freedom
1	A comparison of self-employed and private-sector employees in San Francisco	$F_{503}^{20} = 3.54^*$
2	A comparison of self-employed and private-sector employees outside of San Francisco	$F_{1,747}^{21} = 2.48^*$
3	Self-employed in San Francisco compared to self-employed outside of San Francisco	$F_{409}^{20} = .92$
4	Private-sector employees in San Francisco compared to private-sector employees outside of San Francisco	$F_{1,832}^{24} = 1.58^*$

\*  $p \leq .05$ .

in an ethnic-enclave economy can lead to returns to human capital that equal those received by immigrants in the outside economy.

We examine these results more closely in Table 10. Chinese private-sector employees in San Francisco are clearly disadvantaged compared to the other groups in terms of earning-returns to human-capital characteristics. The Chinese in San Francisco receive no return to years of labor-market experience, while Chinese immigrants residing elsewhere typically experience increased earnings due to labor-market experience for 24 years. Further, Chinese in San Francisco get no direct return to education, while an additional year of schooling at the college level is associated with a 4.3 percent increase in earnings among Chinese immigrants living outside of San Francisco. At the mean earnings of Chinese private-sector employees outside of San Francisco (see Table 7), this estimate translates into a \$700 return per year of college education. The approximate average value of a college degree is around \$2,800 in annual earnings. These findings are consistent with those obtained from the Florida samples. Among the immigrant groups, workers employed in the private sector who live in areas characterized by a well-developed enclave economy typically receive lower returns to human-capital characteristics than do their immigrant group counterparts who live in nonenclave areas.

The observed descriptive disadvantage in earnings among Chinese private-sector employees outside of San Francisco compared to non-Hispanic white private-sector employees who also live outside of San Francisco appears

to be partially a function of differences in earning-returns to human-capital characteristics ( $F = 5.17, 25$  and  $9,503$  d.f.,  $p < .05$ ). For instance, the relative returns to occupation vary substantially between the two groups (equations 2 and 3). Also, the cost of being a recent (1975–1979) rather than an older (prior to 1950) immigrant is greater among the Chinese. The other parameter estimates, including education, English-language skills, and U.S. citizenship, are similar across the two immigrant groups living outside of San Francisco.

Table 11 reports the estimates obtained from the analysis of self-employed immigrants in California. Self-employed Chinese immigrants in San Francisco receive about the same returns to human capital as self-employed Chinese immigrants across California (equation 3, Table 9). Further, a comparison of equations 2 and 3 in Table 11 reveals that Chinese and non-Hispanic whites living outside of San Francisco receive close to the same earning-returns to human-capital characteristics ( $F = 1.49, 20$  and  $2,777$  d.f.,  $p > .05$ ). This is very much the same story as in the Florida analyses. The enclave-economy hypothesis is clearly rejected in the case of private-sector employees. Workers in the enclave appear to receive lower returns to their human capital than do immigrant-workers in the outside economy. On the other hand, our findings do not lead to a rejection of the null hypothesis that there are no differences in the earning-returns of self-employed Chinese immigrants in San Francisco and self-employed Chinese immigrants living elsewhere in California. In effect, we have again obtained results consistent with the enclave-economy hypothesis when it is narrowed to only the experiences of self-employed immigrants.

## DISCUSSION AND CONCLUSION

Our findings do not support the view that the immigrant enclave is a special sector of the U.S. economy in which immigrant-workers in an enclave economy "share with those in the primary sector a significant economic return to past human-capital investments" (Wilson and Portes 1980, p. 302). In confounding immigrant businessmen with paid employees, Portes et al. fail to recognize a distinction in returns to human capital between bosses and workers. The enclave-economy hypothesis must be revised to focus on the advantages of ethnic entrepreneurs in enclave economies:

*While immigrant-minority workers in the open economy tend to receive higher returns to human capital than immigrant-minority workers in an ethnic-enclave economy, immigrant-minority entrepreneurs in an ethnic-*

Table 10. Immigrant Employees in the Private Sector in California Who Worked at Least 160 Hours and Earned a Minimum of \$500 in 1979

Dependent Variable: 1979 Earnings (ln)	Chinese in San Francisco		Chinese Elsewhere in California		Whites (Non-Hispanic) <sup>j</sup>	
Equations	(1)	(2)	(3) <sup>i</sup>			
	B <sup>a</sup>	t-ratio <sup>b</sup>	B	t-ratio	B	t-ratio
Intercept	2.365	5.055	3.867	13.033	3.334	24.898
Labor-market experience	.005	.600(ns) <sup>c</sup>	.033	6.527(23.9)	.039	16.626(30.7)
Labor-market experience squared	-.010	-.673 <sup>f</sup>	-.069	-6.863 <sup>f</sup>	-.063	-13.748 <sup>f</sup>
Married	.045	.645	.122	3.094	.182	11.808
Elementary education	.002	.178	.001	.068	.003	.382
High school education	.017	.795	.004	.244	.005	.784
College education	.019	1.085	.043	5.266	.039	11.416
Hours worked (ln)	.945	17.467	.723	21.685	.781	53.497
English skills	-.101	-2.967	-.090	-4.300	-.074	-9.133
U.S. citizenship	-.002	-.023	.029	.695	.007	.425
Ethnic			.005	.320		
Immigrants 1975-1979	-.344	-3.038	-.430	-5.393	-.172	-5.523
Immigrants 1970-1974	-.143	-1.377	-.147	-1.968 <sup>g</sup>	-.080	-2.523 <sup>h</sup>
Immigrants 1965-1969	-.086	-.890	-.081	-1.140	.026	.925
Immigrants 1960-1964	.064	.503	-.025	-.317	.041	1.617
Immigrants 1950-1959	.024	.205	-.092	-1.208	-.003	-.140
Professional specialties	.438	3.197	.244	4.746	-.012	-.541
Technicians	.202	1.257	.079	1.158	-.103	-3.068
Sales	-.218	-1.477	-.183	-2.649	-.185	-7.058
Administrative support incl. clerical	.014	.116	-.151	-1.966	-.332	-10.676
Business, protective, & household serv.	-.237	-1.983	-.345	-5.786	-.544	-17.279
Farming, forestry, & fishing			-.023	-.105	-.401	-8.190
Precision production & craft	-.064	-.496	.037	.572	-.152	-7.030
Operators	-.178	-1.193	-.123	-1.471	-.267	-8.824
Transportation	.161	.722	-.064	-.432	-.218	-5.852
Laborers	.261	1.348	-.139	-1.193	-.369	-8.854
R <sup>2</sup>	.558		.544		.434	

<sup>a</sup> Unstandardized regression coefficients.<sup>b</sup> A  $t$ -ratio  $\geq 1.96$  indicates that  $p \leq .05$ , two-tail test. A  $t$ -ratio  $\geq 1.65$  indicates that  $p \leq .05$ , one-tail test.<sup>c</sup> Years of labor-market experience at which earnings reach a plateau. (ns = non-significant).<sup>d</sup> Variable is omitted. In equation 1, Ethnic has no variance. In equation 3, Ethnic is unwarranted on theoretical grounds.<sup>e</sup> Variable is omitted in equation 1 because there are no cases (see Table 6).<sup>f</sup> The decimal point of the parameter estimate is moved two places to the right.<sup>g</sup> When the selection hazard rate is controlled for,  $B = -.141$  and the  $t$ -ratio =  $-1.860$ .<sup>h</sup> When the selection hazard rate is controlled for,  $B = -.034$  and the  $t$ -ratio =  $-.968$ .<sup>i</sup> This is the only equation in which the direct association between logged earnings and the selection hazard rate is statistically significant ( $B = .124$ ,  $t$ -ratio = 3.238).<sup>j</sup> Outside of San Francisco.

*enclave economy tend to gain returns to human capital similar to immigrant-minority entrepreneurs in the open economy.*

This revision, however, does not have the theoretical significance of the Wilson-Portes enclave-economy hypothesis. Self-employment in an enclave economy appears to be well rewarded for a given set of human-capital characteristics. But this finding does not support the view that the enclave economy is a protected sector of the U.S. economy. Employers typically draw on ethnic solidarity to enforce and maintain sweatshop conditions, including low wages and closure to union organizing. And

though ethnic entrepreneurs can mobilize to curb competition in enclave economies by agreeing not to undercut each other's profits to the point of economic ruin, competition is still sufficiently intense to result in low profit margins. A feature of the enclave economy that has not been sufficiently studied is the relationship between the low-wage labor pool and the low-profit margins of small businesses in an enclave economy.

According to proponents of the enclave-economy hypothesis, a major advantage of enclave employment is that immigrants gain entry into an ethnic network that assists workers in starting up their own businesses. But as

Table 11. Immigrant *Entrepreneurs in California* Who Worked at Least 160 Hours and Earned a Minimum of \$500 in 1979

Dependent Variable: 1979 Earnings (ln)	Chinese in San Francisco		Chinese Elsewhere in California		Whites (Non-Hispanic) <sup>f</sup>	
Equations	B <sup>a</sup>	(1) t-ratio <sup>b</sup>	B	(2) t-ratio	B	(3) t-ratio
Intercept	6.954	4.406	6.386	7.335	5.666	16.351
Labor-market experience	.006	.221(ns) <sup>c</sup>	.002	.127(ns)	.041	5.915(22.8)
Labor-market experience squared	-.024	-.535 <sup>e</sup>	-.017	-.544 <sup>e</sup>	-.091	-6.741 <sup>e</sup>
Married	.264	1.024	.083	.505	.271	5.875
Elementary education	-.029	-.763	-.026	-.805	.002	.107
High school education	.013	.203	-.001	-.021	-.030	-1.726
College education	.040	.805	.035	1.471	.044	5.365
Hours worked (ln)	.379	2.069	.474	4.820	.513	14.934
English skills	-.360	-3.262	-.067	-1.011	-.033	-1.463
U.S. citizenship	.398	1.630	.119	.904	.039	.895
Ethnic			.013	.272		
Immigrants 1975-1979	.257	.677	-.163	-.758	-.436	-5.372
Immigrants 1970-1974	.129	.365	-.285	-1.541	-.327	-4.017
Immigrants 1965-1969	-.160	-.546	-.065	-.363	-.198	-2.726
Immigrants 1960-1964	.353	.895	.065	.316	-.231	-3.586
Immigrants 1950-1959	.090	.262	-.078	-.431	-.243	-4.562
Professional specialties; technicians	-1.041	-2.701	.286	1.866	.131	2.269
Sales	-.109	-.408	-.116	-.889	-.146	-2.783
Business, protective, & household services	-.087	-.326	-.165	-1.015	-.461	-4.907
Precision production & craft	-.016	-.057	-.234	-1.256	-.141	-2.779
Administrative support incl. clerical; farming, forestry, & fishing; operators; transportation; laborers	-.426	-1.187	-.371	-1.834	-.188	-2.956
R <sup>2</sup>		.468		.208		.202

<sup>a</sup> Unstandardized regression coefficients.<sup>b</sup> A t-ratio  $\geq 1.96$  indicates that  $p \leq .05$ , two-tail test. A t-ratio  $\geq 1.65$  indicates that  $p \leq .05$ , one-tail test.<sup>c</sup> Years of labor-market experience at which earnings reach a plateau. (ns = non-significant).<sup>d</sup> Variable is omitted. In equation 1, Ethnic has no variance. In equation 3, Ethnic is unwarranted on theoretical grounds.<sup>e</sup> The decimal point to the parameter estimate is moved two places to the right.<sup>f</sup> Outside of San Francisco.

indicated earlier (note 16), it is unclear how employment in an ethnic-enclave economy compares to the open economy in facilitating an immigrant's moving into self-employment. Among Cuban and Chinese immigrants, self-employment is least common in the enclave areas. Moreover, Cuban and Chinese immigrants in an out of the enclave economy are less likely to be self-employed than non-Hispanic white immigrants (see tables 2, 3, 7, and 8.) The long-term development of most populations of organizations is constrained by the principle of competitive exclusion (see Hannan and Freeman 1977, especially pp. 939-46). An enclave economy can support only so many entrepreneurs. Success of small businesses depends, substantially, on the maintenance of a large pool of low-wage workers.

The ethnic-solidarity school emphasizes the positive influences of ethnic solidarity on the socioeconomic attainment of immigrant-minority groups, while it ignores many of the negative consequences of ethnic solidarity. In a study of the Chicago Chinese enclave, Li (1977) showed that paternalistic ethnic "assistance" can have a negative effect on subsequent socioeconomic achievement. Immigrants who depend on kinship or ethnic group assistance in the initial stage of adaptation to a host society may become entangled in a web of obligations that interferes with their rational pursuit of economic opportunities. The "embeddedness" of economic activity in networks of ethnic relations can trap immigrant-workers in patron-client relationships that bind them, in exchange for assistance at an early stage, to low-wage

jobs.<sup>18</sup> A detailed analysis of the actual pattern of exchange between bosses and workers within immigrant enclaves is needed before generalizations can be made about ethnic solidarity's effect on the socioeconomic mobility of immigrant workers.<sup>19</sup>

The ethnic-solidarity school has also failed to adequately take into account the extent to which ethnic enclaves are rife with competition and factionalism. Ethnographic accounts of ethnic enclaves provide ample evidence of intense competition and factionalism that render ethnic-based collective action problematic (Whyte 1943; Gans 1962; Suttles 1968; Nee and Nee 1986). Factionalism is reflected in conflicts between rival youth gangs, secret societies, and ethnic-based political organizations. The highly publicized case of the Chung Pak housing project in New York City's Chinatown represents an example of how such rivalry can hurt the quality of life in ethnic enclaves. Here the influence of ethnic divisions threatened to undermine a badly needed apartment building for senior citizens (see *The New York Times*, July 20, 1986, p. 18). In divided ethnic enclaves with a history of subordination, the perception of zero-sum competition heightens the problem of collective action. Collective economic action is highly problematic even when strong rational interests are involved, due to the "free rider" dilemma (Olson 1965). When ethnic solidarity is mobilized, it is typically in reaction to interethnic competition (Hannan, 1979). Such instances of ethnic collective action are usually temporary and difficult to channel into economic action that benefits the enclave as a whole.

In *Economy and Society*, Weber ([1922] 1978, p. 359) characterized the high degree of solidarity of the family unit as "household communism," in which family bonds generate "solidarity in dealing with the outside and communism of property and consumption of

everyday goods within." Due to this quality, the family unit comprises a strategic resource in immigrant adaptation that is often neglected in current studies of immigrant incorporation. Both the qualities of solidarity and communism become valuable social capital in facing the uncertainties and challenges of adaptation to a new society. The strength of family bonds may also allow kinship ties to be used for economic adaptation. Particularly in the case of immigrants who initiate small businesses, the family household provides essential unpaid labor for thinly capitalized businesses to start up and compete. In cases where immigrants enter into wage labor, household communism may likewise provide the basis for family strategies that require individual sacrifices. It may be that reliance on the family household for economic action intensifies the solidarity of the family unit. Research on immigration needs to incorporate an emphasis on family solidarity and household strategies to gain a better understanding of the mix of resources—ethnic, class, and family—that immigrants draw on in their adaptation and of the consequences these resources have on their subsequent socioeconomic attainment.

In an earlier paper, we point to the importance of distinguishing between the cost of ethnicity and the cost of immigration (Nee and Sanders 1985). Previous studies frequently compound these costs, so that the expected socioeconomic dislocations associated with immigration are inadvertently attributed to the cost of ethnicity. It is equally important to distinguish between the cost of immigration and the cost of segregation. There is need for a systematic study of the cost of segregation that works independently of ethnicity and immigration to gain a more accurate understanding of the processes through which contemporary immigrant groups become accommodated to U.S. society.

Though we have not directly tested Park and Burgess' theory, many of our findings are consistent with assimilationist arguments and demonstrate the continuing relevance of this perspective. Both private-sector employees and self-employed Cuban and Chinese immigrants living outside of the ethnic enclave have higher socioeconomic status and are more similar in their educational and occupational characteristics to non-Hispanic white immigrants than are immigrants living in enclave areas. Moreover, immigrant-workers in the open labor market appear to gain higher returns to human capital than immigrant-workers of similar qualifications in the ethnic enclave. These findings conform to assimilation theory in that (1) an inverse relationship exists between immigrants' socioeconomic achievement and their spatial concentration in ethnic enclaves; and (2) lower levels

<sup>18</sup> Granovetter (1985) provides an excellent discussion of why it is important for researchers to be sensitive to the symbiosis between economic behavior (such as the operation of a business) and the interpersonal relationships among actors (e.g., bosses, employees) who take part in the behavior.

<sup>19</sup> In their recent study of post-1980 Cuban and Haitian immigrants, Portes and Stepick (1985) write that they discovered a new and distinct "informal" sector of the labor market made up of immigrant minorities who work for sub-minimal, illegal wages. We suspect that what they identified as the "informal" labor market is an extension of the enclave economy. The immigrant-enclave economy provides an employment niche for both legal and illegal immigrants. Illegal immigrants may find a haven in the enclave economy, but the cost of protection from the Immigration and Naturalization Service may be acceptance of illegal sub-minimal wages.

of cultural assimilation are related to lower socioeconomic achievement.

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# WHAT'S AN ETHNIC ENCLAVE? THE CASE FOR CONCEPTUAL CLARITY\*

(Comment on Sanders and Nee, ASR, this issue)

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In the years since the publication of the original article on immigrant enclaves in the *American Journal of Sociology* (Wilson and Portes 1980) there has been some interest in the concept, including various attempts at refining it. Several studies have explicitly used the enclave hypothesis and the more general concept of mode of incorporation to explain the diverging socioeconomic performance of Spanish-origin and Asian minority groups in the United States (Bean and Tienda 1987, chapter 1; Bach et al. 1984; Rumbaut 1986; Moore and Pachón 1985, chapter 3). Others have used it in comparative analyses of the emergence of a minority middle-class. In a recent article, Hout (1986), for example, proposes that chances for upward mobility are enhanced by a large segregated ethnic community that provides a captive market for an "enclave middle class" of professionals. The Sanders and Nee article in this issue is, however, the first to examine the hypothesis critically on the basis of census data.

Had their analysis corresponded to the original formulation of the hypothesis, there would not be a need for this comment. Unfortunately, the manner in which "enclave" is defined in this analysis has little to do with the definition of the concept in the writings that Sanders and Nee cite. As a consequence, their results are both correct empirically and irrelevant for testing this hypothesis.

Despite their detailed reading of our past work, Sanders and Nee commit a number of inexplicable mistakes of interpretation. Three of these will be cited. First, the difference between the self-employed and employees within immigrant enclaves is well delineated in the literature they cite. For example, the first part of the relevant chapter of *Latin Journey* (Portes and Bach 1985, chapter 6) is dedicated to the analysis of self-employment among Cuban immigrants, its determinants, and the economic advantages associated with entrepreneurship. All participants in the enclave economy were combined in subsequent analyses for a methodological and a substantive reason. Methodologically, further disaggregation would have based

results of multivariate analyses on too few cases. Substantively, these analyses were conducted at a time when the very concept of an immigrant enclave economy was not well established in the sociological vocabulary and, hence, the authors had to delineate the basic contours of this phenomenon and its significance. In this context, further disaggregation would have obscured the central point of the analysis. At present, once the concept has become better established in the research literature, disaggregation of immigrant employees and the self-employed on the basis of larger samples represents a positive step. However, the finding of a consistent economic advantage for the self-employed is not at all surprising, since this is precisely what was anticipated by the enclave hypothesis and was consistently documented in earlier publications (Portes and Bach 1985; Portes and Stepick 1985).

Second, Sanders and Nee misrepresent our views in statements like "The enclave-economy hypothesis advances a 'separate but equal' proposition, which, if true, undermines a major tenet of assimilation theory." Not so. Enclave workers and businessmen are neither "separate" (because their activities are closely intertwined with the broader economy, nor necessarily "equal" (because new arrivals are often much worse off in terms of occupational status and income). The enclave hypothesis never claimed that perennial confinement within an ethnic community would be the road to economic parity. The hypothesis simply stated that, for newly arrived immigrants, participation in a pre-existing ethnic economy can have positive economic consequences, including a greater opportunity for self-employment. This positive adaptation, in particular among immigrant entrepreneurs, creates the basis for a more successful integration of later generations into American society. *Latin Journey* closes on that note: "Adaptation to American society has always taken place and will continue to do so, but its direction and pace and the manner in which subsequent generations become integrated into the mainstream depend on the modes of incorporation and economic accomplishments of earlier immigrants" (Portes and Bach 1985, p. 347).

The third problem is, however, the most serious and, in our view, the one which invalidates Sanders and Nee's results as a test of this hypothesis. The problem is confounding participation in an enclave economy with living in an ethnic neighborhood. In its popular usage, the word "enclave" does evoke the image of residential concentration, but nowhere in our past writings has it been used in this manner. There are actually several passages which warn explicitly against this confusion:

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Finally, we must also distinguish enclaves from immigrant neighborhoods. Most immigrant groups initially resettle in ethnically concentrated communities and generate a few small businesses to serve immediate, specialized consumption needs. Ethnic neighborhoods fulfill important social support functions, but lack the extensive division of labor of the enclave and, especially, its highly differentiated entrepreneurial class (Portes and Bach 1985, pp. 204-5).

Despite their familiarity with these writings, Sanders and Nee chose to ignore the warning and equate enclave membership with ethnic residential concentration. Enclave participants, in their view, are those who live in the areas where the immigrant population is most numerous, and nonparticipants are those who live elsewhere. By contrast, the research literature that preceded this article has defined enclave participation by place of work: enclave entrepreneurs are immigrant owners of firms in an area where similar enterprises concentrate (Wilson and Martin 1982; Portes and Manning 1986). Enclave workers are employees of these firms. Throughout, the empirical criterion has been ethnicity of owners and employees in places of work, not places of residence.

By arbitrarily redefining enclaves as ethnic residential areas, Sanders and Nee guarantee the direction of their results. Before summarizing evidence to illustrate this point, a brief description will help the reader visualize what is actually taking place. In the Miami metropolitan area (Dade County), a large proportion of the currently estimated 25,000 Cuban businesses are concentrated in the municipalities of Miami and Hialeah. In Miami, these businesses concentrate in the Little Havana section around S.W. Eighth Street (Calle Ocho), while, in Hialeah, they are spread throughout the city. Surrounding these areas of business concentration, there are residential neighborhoods consisting mostly of modest houses and apartment buildings, many old and dilapidated. Immigrant businessmen, professionals, and the better-paid employees who work in Cuban firms in Miami and Hialeah generally do not live there. Instead, these groups have progressively abandoned these original areas of settlement and moved toward more affluent suburbs in Southwest Dade County—such as Westchester and Kendall—and, increasingly, toward North Miami Beach and Broward County (Ft. Lauderdale).

An example is provided by a large firm, studied recently, and which we will call Inter-America Transport. It produces and exports sugar-harvesting equipment and other agricultural machinery to Latin America and elsewhere. The firm's offices and plant are

indeed in Hialeah; a large proportion of its personnel is Cuban, as is the family who owns the company. Only the lowest-paid workers live, however, in the vicinity. The managers, engineers, and technicians who work in the plant, as well as the owners, all live elsewhere.<sup>1</sup> Comparing Cuban immigrants who live in the municipalities of Miami and Hialeah with those who live in suburban municipalities amounts to comparing the areas where the lower-income, working-class population concentrates with those where the higher-income groups have increasingly settled.

The results presented by Sanders and Nee are thus not at all surprising, but they do not bear on the enclave hypothesis. The only relevance that we can find in these results is indirect and supports the original argument: if ethnic enterprise is a vehicle for first-generation upward mobility, as the hypothesis predicts, it is to be expected that successful entrepreneurs, managers, and skilled workers would leave the areas of initial settlement and move to more affluent neighborhoods.

The choice of place of residence as the indicator of enclave participation is all the more surprising because there is actually a better variable in the census data. To be sure, the Census does not provide information on the most appropriate indicator—ethnicity of owners in firms where respondents are employed. However, the PUMS data do contain information on the location of respondents' places of work. It would have been a more appropriate test of the hypothesis to compare Cubans who work in Miami and Hialeah with those who work elsewhere. The comparison would still be approximate because not all firms in these two cities are Cuban-owned. However, the present concentration of enclave firms in these cities makes it far more likely that Cubans who work in Miami and Hialeah would be enclave participants than those who work in other Florida cities.

We have conducted this analysis on the basis of the same census data set employed by Sanders and Nee. The PUMS sample with identical age, sex, and occupational characteristics was divided into three groups: (1) Cubans who live and work in core enclave areas, defined as the cities of Miami and Hialeah; (2) Cubans who work in these areas, but live elsewhere; and (3) Cubans in Florida who both live and work outside of this area. The comparison was conducted separately for employees and the self-employed. This comparison

<sup>1</sup> Ongoing fieldwork by the senior author in South Florida.

is similar to those found in Sanders and Nee's tables 2 and 3 with several differences.<sup>2</sup>

Results of this analysis cannot be presented in detail because of restrictions on the presentation of new data in comments such as this.<sup>3</sup> Nevertheless, they may be summarized briefly here. Residents in the enclave area are indeed at a consistent social and economic disadvantage. However, Cubans who work in the enclave, but live elsewhere, are not at a disadvantage vis-à-vis those who have presumably "escaped" the area entirely. Consider, for example, average 1979 annual earnings among private-sector employees: \$11,254 for those who live and work in the enclave; \$15,012 for those who live and work outside the area; and \$16,863 for enclave employees who live elsewhere. Average years of education, percent college graduates, percent in professional specialty occupations, percent able to speak English, and other variables follow exactly the same trend. Hence, the "disadvantage" for ethnic firm employees that Sanders and Nee reports represents a simple artifact of defining the "enclave" as the area where more recent arrivals and poorer refugees tend to live.

Sanders and Nee extend their analysis to the process of income attainment, contending that enclave participation reduces the expected payoff for individual human capital among minority employees. Again their operationalization of enclave membership is erroneously based on place of residence. We have also followed them there and replicated their analysis among (1) the full sample; (2) Cuban residents of the enclave area, defined as above; and (3) Cuban workers in this area. The analysis was conducted separately for the self-employed and for employees. Results for the latter represent the main point of contention. They will be summarized by noting that the pattern of effects of predictor variables on logged earnings does not register a single significant difference between the full sample and the sample of enclave workers.

Adding a dummy variable representing enclave place of work to the set of human-capital predictors in the full employee sample increases explained variance by less than one-half of one percent. The same result obtains when adding

the interactions of place of work with each human-capital predictor to the additive equation. Thus, our analysis of the PUMS data indicate that Cubans who work in the prime area of ethnic-business concentration in the cities of Miami and Hialeah are not penalized economically, whether they are employees or entrepreneurs. On the other hand, the population living in the low-income housing in the vicinity of this business enclave is composed, to a large extent, of the lesser educated and more recent refugees. Their economic disadvantages are confirmed by our analysis, but are not particularly surprising.

The original research that gave rise to the concept of the enclave was not based on an analysis of secondary data, but on extensive fieldwork. It was this experience that drove home the difference between mere residential concentration and the social networks on which a growing ethnic economy was based. The dispersal of the Cuban population away from the areas of original concentration in the wake of economic success is apparent even to the casual observer. Thus, referring to other Latin American immigrants, a recent report on Miami notes "At first, these immigrants lived in Miami proper, in sections of Little Havana that had been vacated by prosperous Cuban-Americans. . . . Cuban-Americans remained attached to Little Havana, at least as a business center, . . . the progressively shabbier apartment complexes around the Orange Bowl were becoming the territory of those Cubans without money, the *marielitos*" (Rieff 1987, p. 68).

In the absence of first-hand field research, we are unwilling to define limits of the Chinese ethnic economy of San Francisco on the basis of secondary data, as done by Sanders and Nee. However, we have analyzed the available PUMS data following their definition of "enclave." Space constraints prevent presenting these results, which are available from the authors on request. We will simply summarize them by saying that they reproduce, in all the essentials, the pattern reported for Cubans above.

What Sanders and Nee have done is to compare low-income central city immigrant neighborhoods with the more affluent members of the same groups living in suburban areas or elsewhere in the state. Results of such analysis are not surprising. If they wish to call certain residential areas "enclaves," that is their privilege, but this definition has little to do with the phenomenon that we have attempted to describe in past writings. Enclaves do not emerge merely by residential concentration—a pattern common to all immigrant groups—but by the exceptional rise of a number of integrated ethnic firms within a metropolitan area that provide employment for a sizable proportion of workers from the same minority. The phenome-

<sup>2</sup> First, we employed a more straightforward indicator of educational attainment—years completed and percent college graduates. Second, knowledge of English was dichotomized and measured as a percentage rather than as a mean; the variable in the PUMS data is a single four-point Likert item and mean values in this ordinal scale are meaningless. Third, we excluded non-Hispanic white immigrants because this group is not relevant to the present argument.

<sup>3</sup> These results and those summarized below are available from the authors on request.

non must be examined on the basis of information on firms and labor markets, not housing.

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### ON TESTING THE ENCLAVE-ECONOMY HYPOTHESIS\*

(Reply to Portes and Jensen, ASR, this issue)

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Portes and Jensen assert that the results reported by Sanders and Nee (1987) should be disre-

garded because we failed to consider place of work in representing participation in the enclave economy. Readers of our article must find this surprising. We did *not* restrict our representation of the enclave economy to ethnic residential concentration. Had we focused our test of the enclave economy hypothesis on communities that lack an extensive ethnic immigrant division of labor and a large entrepreneurial class, we could better understand Portes and Jensen's criticism. The point is made several times in our article that the analyses of the Cuban enclave economy were conducted with three alternative definitions of enclave participation. These are: (1) residence in Miami and Hialeah; (2) residence in Dade county; and (3) *place of work* and place of residence in the Miami SMSA. Because three-quarters of the Portes et al. (see Sanders and Nee [1987] for several references) Cuban enclave sample resided in Miami and Hialeah in 1979, we felt the use of residence in these communities was justified. However, in response to comments given to us by Portes on an earlier draft of our paper, we conducted separate analyses using residence in Dade county to represent enclave membership. This definition includes Cuban professionals and businessmen who work in the enclave, but live in more affluent suburbs. We then conducted a third set of analyses using place of work and residence in the Miami SMSA to indicate enclave membership. Though problems of sample selection bias render the latter analyses less reliable, a problem we discuss (see note 11 of Sanders and Nee [1987]) but Portes and Jensen ignore, *this operationalization uses the same place of work variable as employed by Portes and Jensen*. We reported that the results obtained from each of the three analyses are virtually identical. Where differences do exist, we report them (see notes 11, 13, and 14). Consequently, Portes and Jensen's charge that our findings arise simply because we compare residents of low-income central city immigrant neighborhoods to immigrants living in more affluent surrounding areas is blatantly false.

Portes and Jensen report that their analysis of census data only partially supports our findings. How can this be, since one of our operationalizations of enclave participation takes into account place of work? Portes and Jensen provided us with the analysis of census data to which they refer in their comment. A number of errors mar that analysis. We documented these problems for Portes and Jensen and they reanalyzed the data. We have not seen the reanalysis, but Portes and Jensen maintain that their results are often at odds with ours. We can provide readers with the results of our analysis using place of work and Portes and Jensen have made the same offer. Readers can judge for themselves on this matter.

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Portes and Jensen downplay our findings that the purported benefits of participating in an immigrant enclave economy apply only to ethnic entrepreneurs. Contrary to the enclave economy hypothesis, we find that ethnic workers in the enclave receive comparatively low returns to their human capital investments. According to Portes and Jensen, such findings are anticipated by the enclave economy hypothesis. We fail to see evidence of this. The hypothesis itself makes no distinction between workers and bosses. Portes and Bach (1985) and Portes and Stepick (1985) are cited to support the contention that the enclave-economy hypothesis has led researchers to investigate and document differences in earning-returns for workers and entrepreneurs. In fact, neither study examines the possibility that such returns differ for workers and bosses. As our article points out, Portes and Bach (1985) report a descriptive difference in the average monthly earnings of workers and bosses, and they undertake a discriminant analysis to help justify the specification of three labor markets. These analyses do not address the question of earning-returns. In the subsequent regression analyses that are intended to test the enclave economy hypothesis, bosses and workers are pooled. Hence, there is no effort to compare the possible advantages or disadvantages that bosses and workers experience due to their interactions with the enclave economy. Portes and Jensen excuse the pooling decision by claiming that "these analyses were conducted at a time when the very concept of an immigrant enclave economy was not well established in the sociological vocabulary" and by arguing that examining enclave bosses and workers separately would have produced samples too small for multivariate analyses. In our article, we expressed some sympathy for their position in regard to early contributions of Portes et al., but such arguments are difficult to accept by the mid-1980s. By this time, the Portes et al. concept of an immigrant enclave economy was well established. Yet the researchers continued the old practice of pooling bosses and workers. The analyses reported in Table 74 of Portes and Bach (1985, pp. 234-35) include a multivariate analysis of 99 workers in the secondary labor market. The analysis of enclave "workers" is conducted on a pooled sample of 75 entrepreneurs and 105 employees. Why 99 cases are sufficient for multivariate analyses and 75 cases are not escapes our understanding. The Portes and Stepick (1985) study shows that the Cuban enclave is important in accounting for why recent Cuban refugees from Mariel have been better able to avoid unemployment and informal employment than recent Haitian refugees. While informative, this finding has no direct relevance

for our critique of the enclave-economy hypothesis.

Two additional points must be mentioned. First, Portes and Jensen claim that the enclave economy hypothesis does not contain a "separate but equal" argument. We take strong issue with this disavowal. The hypothesis states, in part, "Enclave workers will share with those in the primary sector a significant economic return to past human capital investments. Such a return will be absent among those in the open secondary labor market" (see Wilson and Portes 1980, p. 302). Portes et al. emphasize that the enclave economy offers Cuban immigrant workers a protected niche where they can avoid the costs of employment in the secondary labor market and instead receive returns to their human capital commensurate with employment in the primary labor market. According to Portes et al., immigrants within the enclave economy can carry out their work and leisure activities without having to know the language of their host country and without extensive interactions outside their own ethnic group. For this reason, they maintain that the enclave economy hypothesis "directly contradicts conventional predictions" derived from assimilation theory (Wilson and Portes 1980, p. 302). This is the main theme of numerous papers and it is an important component of the analyses and interpretations in *Latin Journey* (1985). The "separate but equal" argument is central to Portes et al. It seems to us that Portes is now stepping away from this position; we think it is a move in the right direction. The published literature makes it possible for readers to draw their own conclusions.

Second, Portes and Jensen suggest that we made errors of interpretation because our analyses are based wholly on secondary data. This criticism seems surprising in light of the ethnographic field research conducted by Nee and Nee (1986) in San Francisco's Chinatown. The boundaries of the Chinese enclave delineated in Nee and Nee (1986) include a core enclave area and new satellites in San Francisco. Work and residence are closely intertwined, especially for enclave workers. The results of this research suggest that, far from being a protected niche, the ethnic enclave labor market should be regarded as an extension of the competitive secondary labor market. Discussions of the findings from the San Francisco field study stimulated our interest in conducting a systematic comparative test of the enclave-economy hypothesis. While Portes et al. suggest that ethnic solidarity between boss and worker, over the long run, provides immigrant workers with conditions of work that replicate the primary labor market, the ethnographic study revealed a different reality, one in which

competitive pressures in the enclave labor market drive down wages to levels even lower than the outside secondary labor market. Ethnic solidarity was used by enclave employers to insulate immigrant workers from union organizers and to perpetuate and reproduce a pliant low-wage immigrant work force, not to help workers to enter into self-employment (see Nee and Nee 1986, pp. 278–319).

In light of the strong case for fieldwork made by Portes and Jensen, we are perplexed by the absence of field observations in the text of the published writings of Portes et al. that might support their assertion that a social mechanism, based on reciprocity, operates in which bosses assist workers in their quest for upward socioeconomic mobility. Instead, their discussion of ethnic solidarity between bosses and workers appears to be largely speculative, based on plausible inferences and not on findings from field research. There is repeated mention of reciprocity and networks of cooperation in Portes et al., but no concrete data or examples to support the assertions of ethnic solidarity. We agree wholeheartedly with Portes and Jensen on the importance of fieldwork, yet remain puzzled by their criticism of our work on this point.

Portes et al., through their various writings on Hispanic immigration, have contributed some of the most imaginative and provocative work on current immigration to the United States. Our interest in testing the enclave economy hypoth-

esis reflects our appreciation of the significance of their work. That our analyses show that the hypothesis is not supported in the case of immigrant employees should be seen as affirming the view of sociology as a science that marks its progress by the successive testing of theoretical hypotheses, not as a denial of the contribution of the enclave economy school to sociological thought.

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# THE FORMATION OF JUSTICE NORMS\*

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*Joining ideas from structural exchange theory and symbolic interaction theory, this paper addresses two questions: (a) How does a justice norm form? and (b) Why do variations among justice norms arise? Exchange theory clarifies the objective, structural, and largely "material" contexts in which justice norms originate. Symbolic interaction theory illuminates the subjective, intersubjective, and mainly "ideal" process through which justice norms emerge. Context variations in structural exchange (whether a setting entails distributive or productive exchange, equal or unequal power-dependence) help account for the formation of specific justice norms: equal opportunity, equality, status-rank inequality, need, and equity. We link our theoretical account to existing data and suggest experimental tests of its key implications.*

Much of the early research on distributive justice and equity (Berkowitz and Walster 1976; Cook and Hegtvædt 1983) was stimulated by two distinct but similar exchange theories, one proposed by Homans (1961, 1974) and the other by Adams (1965). According to these formulations, actor A and actor B in an exchange relationship often engage in interpersonal comparison to assess their respective "profit to investment" ratios. If each party's profit is proportional to her or his investment, then the net reward obtained by each actor will be seen as just. Departures from proportionality will be viewed as unjust and lead to various reactions, including anger and attempts to restore justice.

However, the adequacy of the Homans and Adams theories was challenged by Berger, Zelditch, Anderson, and Cohen (1972), who proposed status-value theory as an alternative approach to distributive justice. They noted that the focus of the social comparison process described by the exchange theories is limited to the "local" A-B relationship. But, they conclude, a comparison thus limited would be "anomic" and meaningless because justice reactions require a stable external reference point ("the generalized referential structure") anchored in the social and cultural world outside of, but impinging on, the A-B relationship. In other words, A and B can meaningfully assess the justice or injustice of conditions in their relationship only in light of a legitimate norm *external* to their relationship. This norm must specify the net reward levels persons like A and B should obtain (e.g., "a skilled laborer ought

to earn a higher wage than an unskilled laborer").

The basic point made by status-value theorists is compelling: the Homans and Adams exchange theories omit the normative basis of justice reactions, an essential element of an adequate theoretical account. This insight has directed major subsequent developments in the area. Cook (1975), who clarified expectations states in justice reactions, assumes an existing distribution rule whose legitimacy is not at issue for the persons evaluating justice. Jasso (1980), who extended justice analysis from the individual to the aggregate level, assumes an existing rule defining "the just share" against which an actual share is compared. Markovsky (1986, p. 826, emphasis added), who extended justice evaluation as a multilevel process, assumes that "there exists a legitimate referential relationship" in a social system linking investments to rewards. And Alwin (1987, p. 86), who tested a model linking objective and subjective components of justice, assumes within a justice evaluator an existing sense of "what is a fair or just reward outcome."

Two questions arise. First, how and under what conditions does a normative standard for justice reactions originate? Though this question has been acknowledged as interesting (Cook 1975, p. 373; Alwin 1987, p. 85-86), it has not yet been answered by exchange theorists.<sup>1</sup>

<sup>1</sup> Taking a tack somewhat similar to the present analysis, Ullmann-Margalit (1977) examines how social norms emerge as solutions to "functional" problems posed by (a) the "prisoners' dilemma"; (b) the need for interpersonal coordination; and (c) the difficulties surrounding status inequality. Her analysis, however, does not link such norms to distributive justice or to the issues raised here. Also, research in the expectations states tradition (Berger, Zelditch, Anderson, and Cohen 1972; Berger, Fisek, Norman, and Wagner 1985; Ridgeway, Smith, and Berger forthcoming) has pursued this problem from a different theoretical point of departure. Finally, an

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Second, how and why do justice norms vary? Different distribution or allocation rules ("rationality," "equity," "altruism," etc.) have been distinguished (Meeker 1971; Emerson 1976). Various justice principles ("equal opportunity," "equality," "status/rank inequality," "need," and "equity") have been contrasted (Eckhoff 1974; Cook and Hegtvold 1983). However, no one has explained how and why different justice norms form. This essay explores these questions.

An approach to justice-norm formation drawing together ideas from structural exchange theory and symbolic interaction theory is advanced below. That these theories are very different, even competing, is not denied. But, while there are points of sharp divergence between them, this paper stresses several important points of convergence. Each perspective sheds light on *part* of the justice norm formation process. Yet, because each perspective by itself illuminates only part of that process, neither point of view is sufficient. Symbolic-interaction theory, as usually formulated, underplays the mixed-motive (cooperative/competitive) character of interaction (Schelling 1960) and the non-normative, largely material bases of power and its use, both of which are integral to the formation of justice norms. Exchange theory addresses just these features of social interaction. On the other hand, exchange theory, as usually formulated, ignores the subjective and intersubjective, mainly ideal aspects of interaction (the meanings, understandings, and situational definitions), which are also essential facets of forming justice norms. Symbolic-interaction theory deals with exactly these facets of interaction. This essay builds some conceptual bridges between the two approaches.<sup>2</sup>

### THE STRUCTURE AND PROCESS OF JUSTICE NORM FORMATION

#### *The Objective Structural Basis: Exchange Theory*

Imagine an ecological setting comprised of a set of tribal chiefs, each of whom is the sole head of a separate island village. Each village cultivates a crop (wheat, barley, corn, rice, etc.). The chiefs often travel by boat between islands, making contact with one another. A given pair of island chiefs might come to form what

Emerson (1972)<sup>3</sup> calls an exchange relation, a longitudinal sequence of "opportunities, initiations, and transactions." Suppose, for example, as depicted in Figure 1-I, chief A1 cultivates barley, a resource valued by chief A2, and A2 raises wheat, a resource valued by A1. Each chief presents the other with an ongoing opportunity for a series of mutually advantageous crop trades. On one occasion, A1 might sail to A2's island to initiate a transaction, which, if consummated, would result in an agreement between the chiefs to exchange a specific measure of barley for a specific measure of wheat. On another occasion, A2 might travel to A1's island to initiate a subsequent wheat-for-barley transaction and so on.

An exchange relation can be understood as a power-dependence relation (Emerson 1962), since both actors are mutually dependent for valued resources. A1's dependence on A2, and thus A2's power over A1, is determined by both the extent to which A1 *values* the resource (wheat) obtained from A2, and the *availability* of that resource outside the A1-A2 exchange relation. The greater the value or the less the availability, the greater A2's power over A1 (and vice versa). Each chief's power is equal to the amount of resistance in the other she can overcome in settling the terms of exchange.

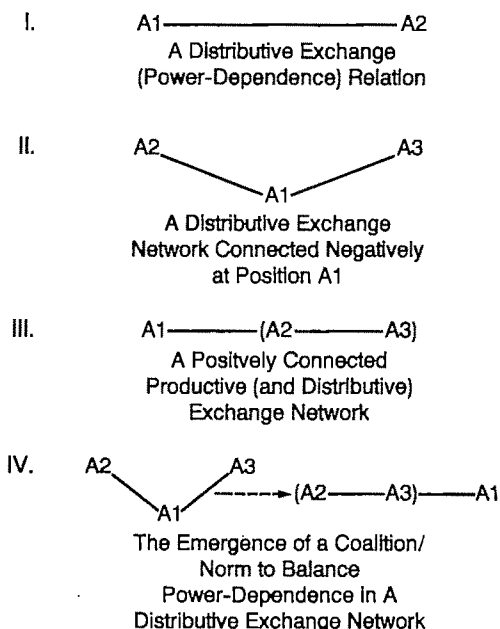


Fig. 1. Contexts of Structural Exchange (suggested by Emerson 1972)

experiment reported by Stolte (1987), on the activation of the equity norm in structural exchange, relates to some aspects of the present analysis.

<sup>2</sup> Other researchers (Stolte 1983; Mutran and Reitzes 1984) have drawn attention to work in sociology and social psychology connecting the two perspectives.

<sup>3</sup> Though the present analysis uses Emerson's exchange formulation, it should be noted that Willer and his associates (Willer and Anderson 1981) have done important work on an alternative structural approach to exchange.

Power-dependence is balanced in an exchange relation when A1 and A2 are equally dependent on each other. In a balanced relation, neither actor will be able to overcome more resistance than the other in settling the terms of exchange. Therefore, the exchange ratio for each actor, expressing the amount of the other actor's resource obtained relative to the amount of own resource given, approaches equality across a sequence of transactions. However, power-dependence is imbalanced when either party is more dependent than the other. The less-dependent party has a relative power advantage and is likely to achieve a more favorable exchange ratio than the more dependent party. If chief A1 were more dependent, she would give increasing amounts of barley to get the same or diminishing amounts of wheat in transactions with chief A2.

Of the two determinants of power-dependence, *availability* has greater sociological significance than *value* because it channels attention toward structural units of exchange that are larger and more complex than the dyadic-exchange relation. For example, availability leads to the conception of an exchange network, a constellation of connected exchange (power-dependence) relations. Consider Figure 1-II. Besides the A1-A2 relation, this structure contains an additional relation, between A1 and A3. Chief A3 might be on another island, where wheat is cultivated and barley is valued. In Emerson's theory, exchange relations are connected when the frequency (probability) of exchange transactions in one relation is a function (positive or negative) of exchange transactions in another relation. A pair of connected exchange relations is an exchange network; such a structure can vary in size. In Figure 1-II, chief A1 has two ongoing opportunities to obtain wheat (from A2 or A3), but A2 and A3 have only one opportunity to get barley. The A1-A2 relation is *negatively* connected to the A1-A3 relation, since the frequency of A1-A2 transactions is negatively correlated with the frequency of A1-A3 transactions (and vice versa). From chief A1's point of view, A2 and A3 are substitutable, alternative sources of the valued resource (wheat). If A1 gets wheat from a given source, the need for it is (temporarily) reduced.

The exchange network situation depicted in Figure 1-II is a "unilateral monopoly," in which chief A1 enjoys an objective position of power-advantage relative to chief A2 and chief A3. By virtue of the availability determinant of power-dependence, A1 is able to overcome more resistance in settling the terms of exchange in a given transaction or sequence of transactions. In an initiation, were A1 to encounter resistance from A2, A1 could seek better terms

from A3, and so on. The theory predicts, and evidence (Stolte and Emerson 1977; Cook and Emerson 1978) shows, that in experimental unilateral monopolies like that displayed in Figure 1-II, A1's exchange ratio will improve, while A2's and A3's exchange ratios will worsen correspondingly. Emerson (1972, p. 77) considers such dynamics the "exploitative use of positional power advantage."

In theory, structured situations of power inequality like that portrayed in Figure 1-II are imbalanced, unstable, and prone to change toward a state of balanced power. One power-balancing mechanism is the evolution of a *positively* connected exchange structure based on "production" (Figure 1-III) from a *negatively* connected exchange network based on "distribution" (Figure 1-II). In Figure 1-II, A2 and A3 provide the same resource (wheat), which has consummatory value for chief A1. Similarly, A1 provides a resource (barley) that has consummatory value for A2 and A3. An exchange relation in which each actor provides the other a consummatory resource entails "distributive exchange." But suppose A1 placed even higher value on wheat bread than on raw wheat. Island chiefs A2 and A3 might form a cooperative relationship in which one chief would grow wheat and the other chief bake it into bread. In such a relationship, the actors provide each other an instrumental (rather than consummatory) resource. The resources are interactively combined to create a joint product. The A2-A3 unit would then, in distributive exchange, give bread for barley in transactions with A1. Members of the A2-A3 unit would then share the barley. The evolutionary changes described here would transform the structured exchange situation in important ways. First, the A2-A1 exchange relation would become positively connected to the A3-A1 relation. Second, A2 and A3 would become cooperators instead of competitors in distributive exchange. Third, power-dependence in the distributive exchange relation between the A2-A3 unit and chief A1 would become balanced, or equalized. In Emerson's theory, a productive exchange relation is a division of labor, a social organization of differential roles or functions (e.g., wheat cultivation and wheat baking), with each role necessary as input to the collective product (e.g., wheat bread).

So far, the exchange processes examined have been entirely *non-normative*. These processes organize and regulate interaction strictly on the basis of underlying exigencies of structural exchange, mutual resource dependence, and dynamics of power. Normative rules and social sanctions play no role in explaining structured social action. However, this exchange theory does deal with social norms in a basic,



important way; it describes the formation of a social norm within non-normative exchange and power interactions.

Consider Figure 1-IV. A second power-balancing mechanism is "coalition formation" among the "exploited" actors (A2 and A3) in an imbalanced distributive exchange network ("unilateral monopoly"). These two island chiefs might create a norm, a shared agreement, governing their transactions with A1. The norm might stipulate that neither member of the coalition will give more than a prescribed amount of the valued resource (wheat) in exchange transactions for A1's resource (barley). Such a norm would delineate the boundaries of an emergent "collective actor," whose members would "act in concert" to pursue a shared goal (better wheat-for-barley exchange ratios). Given such a norm, A1's transactions with A2 would also be implicit transactions with A3, and vice versa. The collective action agreement between A2 and A3 would reduce the number of sources of the valued resource from two to one. Thus, as shown in Figure 1-IV, the power-imbalanced monopoly would be transformed into the power-balanced exchange relation between chief A1 and the A2-A3 collective actor. Each chief would occupy a structurally equal position, and each would be able to obtain an equal wheat-for-barley exchange ratio.

The concepts of structural-exchange theory considered here—power-dependence in relations and networks; distributive and productive exchange; coalition (norm) formation as a power-balancing mechanism—are formulated entirely in objective, behavioral, cognition-free terms. By design, the theory excludes perceptions, expectations, meanings, and understandings. Even in the analysis of social-norm formation, the theory dwells on the structural sources and consequences of action in concert and is silent about what the actors in exchange relations, networks, behavioral divisions of labor, or coalitions might know or communicate. As the author himself acknowledges (Emerson 1972, p. 87), "... this exchange approach is distinctly weak in treating all of the many facets of sociology which, implicitly or explicitly, tie to cognitive psychology." However, at the core of any justice norm is some cognitively apprehended sense of legitimacy. Thus, an adequate account of justice-norm formation requires an expansion of the scope of structural-exchange theory.

#### *The Subjective and Intersubjective Process: Symbolic Interaction Theory*

To clarify how a justice norm forms in a context shaped by power-dependence and exchange, it

will be useful to consider subjective and intersubjective knowledge. I will draw some key symbolic interaction concepts and assumptions (Mead 1934; Berger and Luckmann 1967; Baldwin and Baldwin 1978; Rommetveit and Blaker 1979; Stryker 1980) into the structural-exchange framework.

Again consider chiefs A1 and A2, but this time as depicted in Figure 2-I. Following symbolic-interaction theory, assume that each chief can govern her or his behavior in terms of both *tacit* knowledge, and *explicit* knowledge (Baldwin and Baldwin 1978). To know the objective world tacitly is to apprehend it through direct, personal experience. Such "contingency-shaped" knowledge is private and rich in emotional meaning. For example, landing on A2's island, A1 might intensely desire A2's well-cultivated wheat crop. What A1 would know tacitly is that she sees wheat and wants it. On the other hand, to know the objective world explicitly is to grasp and react to it in terms of verbal and non-verbal signs, the meanings of which are established in human communication (Mead 1934; Vygotsky 1962; Stryker 1980). Through communication, actors translate much of what each knows tacitly (subjectively, privately) into what each comes to know explicitly (intersubjectively, publicly).<sup>4</sup> Thus, chief A1 might not only know tacitly that she wants wheat. She might also know at a higher, symbolically mediated level that A2 is backed by a fierce band of warriors who will protect the crop from being plundered, and, further, that there is no barley on A2's island, suggesting that A2 might value barley. As a result, A1 might attempt to arrange an exchange transaction with A2 of wheat for barley.

Communication at the explicit-knowledge level between the two chiefs, denoted by the dashed line in Figure 2-I, often will be "dialogical" (Rommetveit and Blaker 1979; Markova 1982). That is, meanings transmitted will frequently (but not always) be negotiated (Stryker and Gottlieb 1981). Chief A1 might encode a message with a given meaning. A2 might decode that message accurately, and encode a reciprocal message, which A1 would attempt to decipher correctly. A process of give-and-take continues until the chiefs perhaps (but not necessarily) come to a mutually ratified meaning agreement. For example, they might negotiate the meaning agreement to exchange a

<sup>4</sup> The distinction made by Baldwin and Baldwin (1978) between tacit and explicit knowledge seems to correspond closely to the distinction drawn by Soviet sociophysiological researchers (Creelman 1966; Luria 1981; Vygotsky 1962) between responses made in terms of the "first" versus "second" signalling systems.

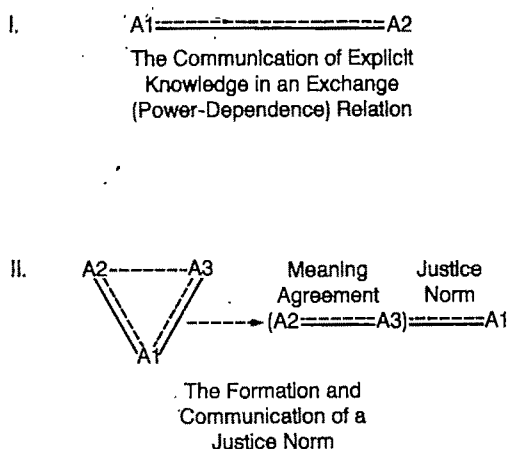


Fig. 2. Symbolic Interaction in Structural Exchange

given measure of wheat for a given measure of barley.<sup>5</sup>

Of course, human actors can, and often do, negotiate (or merely form) meaning agreements of far greater depth, subtlety, and complexity than a simple agreement to exchange crops. Mutual understandings need not be bound directly to what each actor tacitly knows about the other actor or to their concrete, material circumstances of power-dependence and exchange. Meaning agreements may be formed in reference to imaginary events and conditions invented entirely within the linguistic dialogue that takes place. Apart from the utilitarian interest each chief has in trade, A1 and A2 might speak of many purely intellectual issues (e.g., the nature of truth, beauty, art, God, etc.). In short, symbolic interaction to *some degree* frees human actors from the present, concrete, tacitly known space, time, and exchange situation.

However, my present concern is limited to meaning agreements formed in reference to the mundane, tacitly experienced material conditions of exchange and power-dependence. My focus is the process through which actors located in a specified exchange structure form meaning agreements about "what is and can be" and about "what ought legitimately to be." Such

meaning agreements lie at the heart of justice-norm formation.

### *Coalitions and Justice Norms: From "Is" to "Ought"*

Consider again the dynamics of coalition formation in which power becomes balanced and norms emerge, but this time explicitly include the assumptions of symbolic interaction (see Figure 2-II). Suppose A2 and A3 are chiefs from geographically proximate islands, who are acquainted, have frequent contact, and speak the same language. Assume also that conditions on their islands permit only the cultivation of wheat. Because their crops are identical, the two chiefs have no basis for exchanging resources. However, they do have a basis for accumulating jointly understood meaning agreements about "what is and can be," given the prevailing exigencies of structural exchange. An exigency they are likely to consider important is the wheat-for-barley exchange relation each has with chief A1.

Assume, as before, that A1 uses her positional power advantage in a sequence of transactions in the exchange network. As before, exchange ratios will become unfavorable for A2 and A3 and favorable for A1. A1 is using power advantage over the other two chiefs, a fact that A2 and A3 first know tacitly and privately, but later know explicitly and jointly through dialogue. Pooling individual experiences through symbolic interaction, A2 and A3 agree about what is happening and why. They agree that they are subject to A1's power use because she occupies a positional advantage in the network. Under these conditions, a meaning agreement with reference to the acts is likely to be, at least, roughly veridical.<sup>6</sup> Besides forming

<sup>5</sup> A meaning agreement in this sense is a specific mutual understanding. We assume that the interests of two actors in an exchange relation will remain at least partially opposed. Goals are rarely, if ever, perfectly shared. Thus, mutual cooperation concerning goals is a matter of degree, sometimes, but not always, sufficient for the consummation of a resource transaction. Chief A1 and chief A2 might negotiate a meaning agreement in which they come to the mutual understanding that their goals/interests are in utter conflict, thus precluding a transaction.

<sup>6</sup> The argument here distinguishes, but attempts to bridge, two levels: the objective structural level; and the subjective meaning level. These levels might be kept distinct. A researcher might address strictly objective, directly visible actions and their consequences, for example, observing and describing power use as a longitudinal change in exchange ratios to the advantage of some actors and the disadvantage of other actors. It is unfortunate that Emerson (1972, p. 77) uses the term "exploitation" to describe such power use, because the term implies a value judgment in light of some normative standard. Despite such language, however, Emerson's theory is intended as an empirical theory of objective-interaction structure. Building from his theory, the present formulation is also intended as a strictly empirical theory. It attempts to span the objective and subjective levels, showing how persons located objectively in exchange structures subjectively form and communicate perceptions and judgments.

Of course the relationship is complex between what actors *take to be* true facts of structural exchange, and

an agreement about what their joint problem is, A2 and A3 are also likely to create and pursue a joint plan aimed at solving it. A mutual understanding that they can effectively reduce A1's positional power advantage by transacting with her only at a prescribed wheat-for-barley exchange ratio will probably form.<sup>7</sup>

It should be noted that the meaning agreement negotiated by A2 and A3 in this fictional example is quite similar to the contract norm developed through bargaining in power relations studied by Thibaut and his colleagues (Thibaut and Kelley 1959; Thibaut 1968; Murdoch 1967; Michner, Griffith, and Palmer 1971; Michner and Zeller 1972). Their research generally supports much of my argument.

It is crucial to understand *where* the justice norm resides in this social situation. The norm entails, but does not reside directly in, the meaning agreement formed jointly by A2 and A3. Having construed a joint problem, and united to solve it, these actors have jointly ratified an agreement consisting largely of "what is and can be" through concerted action within the exchange structure. For the chiefs to regulate their actions is not costly; each has everything to gain and nothing to lose by cooperating to impose a prescription on A1. Neither prescription nor sanction is necessary to move A2 and A3 to such joint action because such action is in the *self-interest* of both chiefs. Generally, however, a social norm forms with reference to actions that are *costly* and not in an individual's immediate self-interest. These actions are, instead, in a collectivity's interest against the interest of an individual. It is precisely such actions, which would not otherwise be undertaken, that must be encouraged or

compelled by a collective norm. Thus, the justice norm resides specifically in the relationship between the A2-A3 collectivity and chief A1. Within this relationship, a normative prescription and potential sanction will be communicated by the coalition to A1. As focus shifts from the meaning agreement formed within the A2-A3 collectivity to the justice norm communicated by the collectivity, symbolic content is transformed from "what is and can be" into "what ought to be." Justifications communicated by the A2-A3 unit are *not* likely to be framed in terms of A1's self-interest; lowering the exchange ratio is decidedly against A1's immediate self-interest. Rather, such justifications tend to be framed in terms of the "collective interest" over A1's self-interest. In short, that which will be communicated to chief A1 will be a sanctionable and legitimate justice norm.<sup>8</sup>

It is also essential to understand what "legitimate" means here. If chief A1 conforms to the communicated justice norm strictly because she believes (cognitively predicts) that the A2-A3 unit will jointly sanction her (will jointly withhold wheat unless she conforms to the prescribed exchange ratio), even though A1 resists what is communicated, then the justice norm is legitimate in the sense of being *valid*. If A1 conforms both because she attributes validity to the norm and endorses it, the justice norm is also *proper*. A collectively communicated prescription becomes a justice norm when, as a minimum condition, it is taken as valid. Beyond this essential minimum condition, a justice norm might also become proper under certain conditions.<sup>9</sup>

what the true facts *are* by an objective, scientific standard. There are many possible sources of error, psychologically and intersubjectively. Individuals often make "attribution errors" (Nisbett 1980; Tversky and Kahneman 1974). And intersubjective communicators frequently fail to arrive at a "parallel account," or accurate reflection, of objective situational facts (Rommertveit and Blakar 1979). However, while meaning agreements are often mistaken, they also are often correct. A veridical meaning agreement would be particularly probable in a stable, longitudinal exchange network like that assumed in this illustration. In such a situation, many tacit "tests" of hypotheses, as well as many opportunities for the joint negotiation of attributions, are possible.

<sup>7</sup> It must be recognized, of course, that outcomes other than that described in this hypothetical illustration are possible. For example, A2 and A3 might not form a meaning agreement at all. Instead, they might continue competing with each other by exchanging individually with A1, to their joint detriment. Or, as discussed below, A2 and A3 might form different kinds of meaning agreements (and justice norms), depending on differences in the exigencies of exchange.

<sup>8</sup> The contrast here between the A2-A3 meaning agreement about "what is and can be" and the justice norm communicated by the A2-A3 unit to A1 about "what legitimately ought to be" is related to Simmel's distinction (Wolff 1950) between the dyad and larger groups (triads, etc.). Chief A2 and chief A3 each know that their joint meaning agreement is merely interpersonal. They perceive that such an agreement is based only on each other's self-interest, and that it might well dissolve if their respective interests change. But chief A1 knows that a justice norm communicated by A2 is impersonal in the sense that it is anchored beyond A2 in A3's reaction as well. Such a norm is thus "superindividual" in its objective, exterior facticity.

<sup>9</sup> Dornbusch and Scott (1975) distinguish validity and propriety as different dimensions of legitimate authority. Also see Thomas, Walker, and Zelditch (1986). A justice norm that is valid but not proper is close to or synonymous with Weber's *herrschaft* (institutionalized domination). In this regard, see Emerson (1981).

It may be that a justice norm initially taken as merely valid will also become proper from an actor's perspective through the social psychological process of internalization. See Stolte (1978) for a discussion of this topic.

This approach suggests that justice-norm formation involves the forceful imposition of cost. There is a sense in which "what legitimately ought to be" is a matter of "what is forced to be" (or threatened to be). But concepts like "force" and "cost" are quite broad. In the illustration, the A2-A3 collectivity forces A1 to accept the lower exchange ratio through the use or threat of deprivation. If A1 will not exchange at the prescribed exchange ratio she will be deprived of the resource she values.<sup>10</sup>

For exchange-structural reasons, the members of a newly empowered collectivity are often (but not always) motivated to legitimize (validate) their structural position. That is, they will be moved to transform what to them "is and can be" into what is legitimate from A1's perspective. In this illustration, the A2-A3 collectivity will be motivated to transform the objective power it holds into what A1 takes subjectively to be legitimate power. Included among the structural bases of such motivation is the fact that, under certain conditions, power-dependence is *potential*, advantageous to have but costly to use. Through joint action, A2 and A3 can potentially deliver sanctions to A1: they can affect A1's experience by denying her the valued resource she seeks. But A2 and A3 will themselves experience the cost of doing without the resource (barley) they value. To use power-dependence will be costly to the coalition as well as chief A1. However, if the coalition were to use power-dependence only to the point necessary to get A1 to validate their communicated justice norm, they would obtain A1's conformity at much less cost in power use than in the absence of such a norm.<sup>11</sup> Indeed, if the justice norm were also to become proper from A1's point of view, the costs borne by the A2-A3 coalition to obtain A1's conformity

(e.g., the costs of monitoring A1 closely) would be reduced even further.<sup>12</sup>

### STRUCTURAL VARIATIONS AND THE FORMATION OF DIFFERENT JUSTICE NORMS

Above, I tried to answer the first question: How do justice norms originate? Now, in light of this discussion, I address the second question: How and why do different justice norms form? My goal is to explain how and why Eckhoff's (1974) five "justice principles" (*equal opportunity*, *equality*, *status/rank inequality*, *need*, and *equity*) are likely to form. While not exhaustive, his list contains some of the most important justice norms studied empirically (Cook and Hegtvold 1983). Below, connections are drawn between these justice norms and several of the non-normative contexts of structural exchange delineated in or suggested by Emerson's structural exchange theory. Justice norms form through the general process described above. But the specific character and content of a given justice norm depends on which of several possible sets of structural-exchange exigencies prevail.<sup>13</sup>

#### *The Context of Distributive Exchange*

*Power balance: equal opportunity and equality.* The emergence of these two justice norms can

An important issue, often discussed by legitimacy theorists, comes up in the distinction between validity and propriety (endorsement) as bases of legitimacy: how stable are structures resting on validity alone? While an answer to this question lies beyond the scope of the present analysis, the issue is a worthwhile focus for future research.

<sup>10</sup> "Force" or "cost" might alternatively subsume coercion, as well as deprivation. Under certain conditions, as discussed below, a collectivity might actively impose or threaten to impose more or less extreme negative events, including violence.

<sup>11</sup> This point fits generally with the ideas and data advanced by Thibaut and his colleagues in the "power-leads-to-norms" research, discussed above. Also, the distinction drawn here between power use through the conveyance of a tacitly experienced sanction and an explicitly known justice norm parallels the distinction Jones and Gerard (1967) draw between influence based on "effects" and influence based on "cues."

<sup>12</sup> This discussion of the structural sources of legitimation must be qualified, however. An important condition surrounding legitimation is the specific nature of the structure of power-dependence. The "cost-of-power-use" mechanism is likely to stimulate efforts to legitimize among coalitions of actors who have power-dependence that is equal to those they wish to influence, like the situation described in the example in which the A2-A3 coalition is equal to A1. However, an alternative general possibility, elaborated below, is the case of a coalition with a structurally based and stable power advantage. In this instance, the members of the coalition are motivated to use power over the subordinated actor(s), but are not motivated to legitimize their position. Nevertheless, their position may become legitimate, not through their own actions and words, but rather through mechanisms operating among the subordinated actor(s). One mechanism, for example, might be a lowering of self-evaluation among those who are subordinated and a consequent acceptance of a relatively deprived position as "right, reasonable, legitimate" (Della Fave 1980).

<sup>13</sup> My point of departure is somewhat similar to that of other researchers. For example, Michner, Griffith, and Palmer (1971, p. 230) state: "... the balance of power between bargainers becomes important, and the morphological structure of emergent contractual norms will mirror the underlying power capabilities of the negotiating parties." Also, Deutsch (1985, p. 44), arguing that "... the typical consequences of a given type of social relation tend to elicit that relation," shows how different bases of goal interdependence lead to the activation of different justice rules.

be accounted for by extending the above discussion of the power-balancing coalition. The exigencies of structural exchange in this case lead to the formation of a collectivity that objectively balances and equalizes power-dependence in a distributive exchange network. That is, power equality comes objectively to characterize the exchange relations among actors in the social situation. As discussed above in relation to Figure I-IV, chief A1's previous monopoly will be eliminated, if chiefs A2 and A3 form a coalition. With coalition formation, each actor will have equal exchange opportunity, which, in turn, will lead to equal exchange ratios by all. Thus, the "is and can be" substance of the A2-A3 meaning agreement is likely to reflect the facts that emerge in the situation. When the A2-A3 collectivity communicates a justice norm to A1, what *can* be effected through joint action will be translated into what *should* be effected. So, the A2-A3 coalition is likely to communicate "equal opportunity" and exchange ratio "equality" as prescribed conditions.

*Power imbalance: status/rank inequality and consummatory need.* While Emerson's exchange theory deals with coalition formation in only one limited context (power-balancing in a distributive exchange network), collectivities may form under a variety of structural exigencies in an exchange network to pursue different purposes with different effects. Another possibility is the formation of a coalition to create a *power-imbalance* to the advantage of the collectivity. There are two major variants. First is the "predatory" or "exploitative" coalition aimed at achieving neither equal opportunity nor exchange ratio equality but, rather, domination. By coalescing, the members of such a collectivity are able to improve their exchange ratios far beyond equality at the expense of others in the exchange-structural environment. For example, several island chiefs who together can muster a strong band of warriors might subjugate surrounding island villages and simply appropriate the crops they desire. It is likely that the "is and can be" content of the meaning agreement negotiated by coalition members (i.e., a superordinate position in a factual structure of inequality), will be accepted by subordinated chiefs as a valid condition of social life in the form of the justice norm *status/rank inequality*.<sup>14</sup>

Second, there is the "protective" or revolutionary" coalition (Lawler 1975). Again, the goal of such a collectivity is neither equal opportunity nor equality. In this instance, the collective goal is likely to be the *redistribution* of consummatory resources, taking from actor who previously have enjoyed a structural advantage and exchange-ratio privilege, giving to those who have previously suffered structural disadvantage and exchange-ratio deprivation. In translating the "is and can be" into the "ought legitimately to be," the members of such a coalition are likely to formulate "consummatory need" as a justice norm: "From each actor in the exchange network according to her accumulated privilege, to each actor in the structure according to her (subsistence) need," regardless of such other considerations as equality versus inequality, merit versus incompetence, etc.

### *The Context Of Productive Exchange*

*Division of labor: equity and instrumental need* Finally, turn from distributive to productive exchange as discussed earlier in relation to Figure 1-III. In reconsidering this situation however, assume that an additional actor, A4 another island chief, is part of the exchange structural situation. Suppose that A2, A3, and A4 all initially cultivate wheat, which each separately trades for barley from A1. As noted above, A1 would therefore occupy a structurally advantaged position in a distributive-exchange network. But suppose that A2 and A3 accurately discover, through symbolic dialogue, the power-balancing benefits that would accrue to them were they to unite in a productive-exchange relation to make wheat bread. Assume that a successful joint venture will require three functions: wheat cultivation, wheat milling, and bread baking. But A2 and A3 can handle only one of these functions each. It is likely that A2 and A3 will induce A4 to join them in an emergent division of labor. The exigencies of exchange in such a structure dictate that each member of the collective actor would benefit in direct proportion to the value of the collective product (wheat bread). That is, the more valuable the bread A2-A3-A4 can turn out, the more barley they can, as a unit, demand from A1 in trade. Thus, from the shifting perspectives of any pair of actors in relation to a third actor within the coalition formed for productive exchange (A2-A3 standing above A4, A2-A4 standing above A3, or A3-A4 standing above A2), it would be *objectively* in the collective

<sup>14</sup> A case study of exactly such a situation occurred in Baltistan (Emerson 1981, p. 62), where Muslims banded together in 1400 A.D. to subjugate a population of Buddhist farmers, maintaining unbroken domination until 1972. The subordinated peasant farmers gave two-thirds of their agricultural yield and their labor in unquestioned obedience to the commands of rulers. It is likely that across the nearly 600-year period, "status/rank inequality"

ity" was communicated to or, at least, accepted by the farmers. Further, see the case Carneiro (1970) builds to support "coercive" over "voluntaristic" theories of the origin of the state. Finally, see footnote 12.

interest to encourage the contribution of instrumental resources (cultivating, milling, and baking) of maximum value. Further, it would be in the collective interest to discourage "free riding," that is, an actor's taking a larger share of the consummatory resource (barley) received by the coalition than would be warranted in light of the value of her contribution to the collective product. It is likely that any pair of actors in the coalition will communicate to the remaining actor the justice norm *equity*, which stipulates that each actor should legitimately obtain an exchange ratio (i.e., a measure of barley) in direct proportion to the value of the instrumental resource she has contributed to the collective product. Further, each normative pair standing above the remaining actor would likely communicate a justice norm that might be called "instrumental need." This norm would prescribe that resources contributed to the collective product be used so as to maximize their instrumental value. In light of such a norm, it would not be permissible for an actor to consume any portion of the instrumental resource (cultivated wheat, milled wheat, or baked bread). Also in light of such a norm, it is expected that instrumental resources would be managed by all members in such a way as to maximize efficiency and quality of production.

#### DIRECTIONS FOR RESEARCH AND CONCLUSIONS

While this account seems plausible and accords with some of the evidence,<sup>15</sup> it must be tested more directly and refined empirically. The following hypotheses suggest some fruitful directions for research, though they will require further specification before data collection. The first prediction concerns whether or not justice norms are likely to form:

**HYPOTHESIS 1.** *Members of a coalition who have power-dependence equal to those they wish to influence will be more likely to formulate and communicate justice norms than members of a coalition who have power-dependence greater than those they wish to influence.*

Next, these predictions relate to the content of justice norms:

**HYPOTHESIS 2.** *Members of a coalition*

*engaged in productive exchange will tend to form and communicate equity and instrumental need as justice norms.*

**HYPOTHESIS 3.** *To balance power-dependence that has been imbalanced, the members of a coalition engaged in distributive exchange will tend to form and communicate equal opportunity and equality as justice norms.*

**HYPOTHESIS 4.** *In distributive exchange, given a prior history of being subject to power use by advantaged others, and given the emergent attainment of power advantage, the members of a coalition will tend to form and communicate consummatory need as a justice norm.*

**HYPOTHESIS 5.** *Given a stable imbalance of power-dependence, the members of a subordinated coalition will form and use status/rank inequality as a norm to justify their relatively deprived position.*

One way to proceed is to evaluate hypotheses such as these experimentally. Emerson and his colleagues (Stolte and Emerson 1977; Cook and Emerson 1978; Cook et al. 1983; Stolte 1983; Cook forthcoming) have reported techniques for the controlled laboratory study of distributive exchange in networks and productive-exchange systems. Procedures they described are a starting point. However, these procedures require modification so that symbolic interaction could be explicitly monitored as effectively as processes of resource exchange. For example, in a laboratory, a small group of subjects could interact via microcomputers in a network. It would be feasible to experimentally manipulate various features of exchange structure; it would also be possible to vary aspects of explicit knowledge. Then, in addition to intersubject exchange transactions, intersubject communications could be monitored and analyzed. It would be possible to create in the laboratory the essential features of the unilateral monopoly and examine the meaning agreements subjects form through dialogue. Justice norms formed and communicated by coalitions could be observed. Using such procedures to test the hypotheses listed above would provide an empirical basis for refining and extending the proposed theoretical argument.

In summary, I attempted to use exchange theory and symbolic-interaction theory to account for the justice-norm formation process. This process leads to the development of distinct justice norms, the contents of which depend on variations in the structural context of exchange. I hope my effort suggests the promise of exploring the mutual relevance of theoretical approaches that seem to be unrelated or even

<sup>15</sup> The "power-into-norms" data reported by Thibaut and others, discussed above, support the idea that norms are negotiated under conditions of varying interdependence to regulate power use. The data reviewed by Cook and Hegtvædt (1983) and Deutsch (1985) broadly support the notion that different structural conditions stimulate the formation (or activation) of different justice norms.

opposed (Stryker 1977). I believe that when frameworks, such as structural-exchange theory and symbolic-interaction theory, can be joined to address a topic, they should be. A richer understanding can emerge from their convergence.

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# DISORDER IN THE LIFE COURSE: HOW COMMON AND DOES IT MATTER?\*

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*Many researchers conceptualize the major transitions in the life course as occurring in an orderly progression. Using data from the National Longitudinal Survey of the High School Class of 1972 and its follow-ups, we find considerable disorder in nonfamily events. By the time they had been out of high school eight years, over half of the men and women in the 1972 class had sequences of states that deviated from the "normal," e.g., they returned to school or moved to a category not usually included in life-course research, such as unemployment. To what extent does this disorder matter? We answer this question with respect to the transition to parenthood. A simple measure of disorder did not uniformly affect this transition. Rather, disorder has heterogeneous effects. Particular sequences of activities, some orderly, some not, affect when people first became parents. For example, the delaying effect of education is less powerful if that education has been interrupted by work or some other activity.*

## INTRODUCTION

Elder (1985, p. 3) has stated, "Life patterns are structured by variations in the timing, duration, and order of events; and by the interlocking careers (in family, work) that vary in synchronization." Virtually all individuals in industrial and postindustrial societies undergo a variety of important role transitions, such as entry into school, entry into the labor force, entry into parenthood, exit from school, and retirement from the labor force. Because the roles themselves (e.g., worker or parent) are essential to the functioning of society, movement in and out of them has been a core sociological concern for at least half a century, since Loomis and Hamilton (1936). In this paper we explore the patterning of early adulthood by examining the ordering of activities that make up nonfamily careers and how that ordering affects the timing of family transitions, in particular the transition to parenthood.

The life patterns of men and women are always conditioned by their historical context. The disruptive effects of the Great Depression and World War II provide perhaps the best documented examples (Elder 1974; Hogan 1981; Modell 1980), but substantial change in the age at which various transitions occur has been taking place throughout the twentieth century. The amount of schooling received has increased; hence, age at leaving school has increased. Variation has decreased in the ages when individuals obtain their first full-time job and enter the military. In general, many events typically used to define the transition to adulthood have become concentrated in a shorter period of time and now occur at somewhat younger ages than in the past (Featherman et al. 1984; Modell et al. 1976; Winsborough 1978). Role transitions continue to be age-graded, and many of the more important ones tend to occur during an individual's late teens and twenties. The concentration of important events in this age interval has increasingly rearranged school, work, and other events into a variety of sequences.

Both the timing and the order of role transitions are important (Elder 1975; Elder 1978; Hogan 1978; Neugarten et al. 1965), but the former has received far more empirical attention than the latter. It is often suggested that appropriate ages exist for making these transitions, and that they ought to be made in an appropriate sequence (Elder 1974; Modell 1980; Neugarten et al., 1965). While there has been some debate on whether these expectations about the sequence of transitions are norms in the classic meaning of the term (see Marini

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1984a), both individual actors and social scientists have expectations about the order of these life-course events, even if sanctions are not applied. In fact, many sociological theories build in an expected sequencing of events in the transition to adulthood. For example, despite evidence to the contrary, first marriage is still sometimes equated with the beginning of exposure to the risk of parenthood. This is stated explicitly in a recent article by Teachman and Polonko (1985), and age at first marriage has received much attention elsewhere. Another example is the typical status-attainment model, which assumes that education will end before one's first job (e.g., Campbell 1983; Featherman and Hauser 1978; Sewell et al. 1969; Sewell and Hauser, 1975). Quite often, such an assumption is built directly into data collection procedures. The Occupational Change in a Generation II (OCG-II) data set phrased the question on first job such that respondents were to provide their first full-time job *after* they completed their formal education.

Despite the persistent theme of orderliness and irreversibility in the transition to adulthood, especially with the accumulation of life-history data, the life-course literature suggests that the time after high school is actually fluid and reversible (Dannefer 1984; Featherman et al. 1984; Marini 1987; Rossi 1980; Sørensen 1984). Foner and Kertzer (1978) have discussed the individual and societal consequences of the flexibility of the American system relative to age-set societies, emphasizing the importance of the role rehearsals that part-time and summer work provide. Hogan (1978) finds that, even with the explicit attempt in the OCG-II data collection to impose order, over 12 percent of the men reported beginning their first job before they left school. Using data that did not try to impose an order on finishing education and entering the labor market, Featherman and Carter (1976) show that 40 percent of the men in their sample interrupted their schooling after high school. Davis and Bumpass (1976) and Marini (1984b) report that one-fifth of the women in their two different studies attended school after marriage.

There are also hints that such disorder (relative to conventional models) affects later life transitions and outcomes. Hogan (1978), for example, finds disorder leads to higher rates of marital disruption, and Featherman and Carter (1976) show that interrupted schooling lowered income in one's early 30s.

Literature is growing on the effects of events or processes in one sphere (family or nonfamily) on outcomes in the other. For example, a number of studies have examined the causal relationship between schooling and labor-force participation on the one hand and marriage or

fertility on the other (Cramer 1980; Haggstrom et al. 1981; Marini 1984c; Rindfuss et al. 1980; Smith-Lovin and Tickamyer 1978; Waite and Stolzenberg 1976). However, when examining the sequence of life-cycle events there has been a tendency for researchers to commingle familial and nonfamilial events. "Normative" or "expected" order is usually presented as leaving school, entering the labor force full time, getting married, and then having the first child (Hogan 1978; Marini 1984b). It is almost as if parent and student were mutually exclusive roles.

The problem with this past strategy for sequencing is that individuals can simultaneously hold both family and nonfamily roles. Indeed, young adults are sometimes encouraged to marry and even have children before they leave school. Doctoral candidates, for example, might be expected by their parents and others to marry before they complete their education. Similarly, working mothers have become almost as common as working fathers. Further, as Marini (1987, p. 12) argues, "It is important to recognize that the role changes occurring during this period [late adolescence, early adulthood] differ from each other in substantively important ways. . . . These substantive differences require that attention be focused on transitions into and out of particular roles rather than on transitions between states defined by combinations of roles, where a transition results from a change in any one of several roles." Both work and family spheres have expected transition sequences. For example, marriage is expected to precede parenthood, and the completion of schooling is expected to precede full-time employment. But each sphere also permits a variety of sequences, especially in contemporary U.S. society. Given that people occupy both spheres and that theoretical confusion is possible if family and nonfamily roles are combined into analytical categories, our strategy is to first focus on order or disorder within one of the spheres (family or nonfamily). If disorder exists, we then examine the effect it has on the other sphere.

Further, prior work on sequencing has tended to gloss over many potentially critical aspects of the transition from student to worker. For example, one or more spells of unemployment may be involved, and the unemployment may bring about a return to school. A year abroad before settling down is a fantasy of many college students and the reality for a nontrivial minority. Military service can occur at any point; for some it may provide the financial basis for a return to school, while for others it may turn into a long-term career. A much richer specification of activities than has been the case

is needed to see the full effects of order and disorder.

We examine orderliness in the school-work sphere for young men and women making the transition to adulthood during the 1970s. We then examine the extent to which disorder in this sphere affects the timing of the transition to parenthood in the family sphere. While work-school transitions can be reversed (e.g., a return to school after full-time work), one normally cannot reverse having become a parent. The transition to parenthood is thus a particularly good "dependent" variable in the study of effects of disorder. In addition, there is a long literature showing that the timing of this transition has important subsequent implications for both the new parents and the child. Further, if people have children when they are in their 20s, regardless of what they are doing outside of family life, we are less likely to find disorder influences. Our analysis is thus a conservative one.<sup>1</sup>

In testing for disorder effects, we also test for a weaker form of process-history effect. Throughout the paper, we allow nonfamily activities at the immediately prior time,  $t-1$ , to affect the transition to parenthood at the subsequent time,  $t$ . To test whether the process forgets its more distant past, we see whether activities at substantially previous times (i.e.,  $t-2$ ,  $t-3$ , etc.), additively affect the timing of the transition to parenthood. To see whether disorder is important, we examine the effects of particular sequences of previous nonfamily activities on the timing of first parenthood. To preview our results, we find the existence of substantial disorder, that the history of activities is important in predicting parenthood, and that, beyond this, particular sequences of activities affect the transition to parenthood.

## DATA

We use data from the National Longitudinal Survey of the High School Class of 1972 (NLS) (Center for Educational Research and Evaluation, RTI, 1979). The initial interviews took

place in the spring of 1972, when the respondents were in their last year of high school. Follow-ups were conducted in the fall of 1973, 1974, 1976, and 1979 (Riccobono et al. 1981). The NLS is an excellent data set for our purposes, not only because of the large sample (over 20,000 cases) and the richness of the data, but also because both men and women are included. Further, the longitudinal character of the study minimizes recall error.<sup>2</sup>

Approximately three-quarters of the members of this study were born in 1954, and 20 percent were born in 1953. During their years in grade school, the civil rights controversy was at its peak. During their high school years, the Vietnam War and feminism were major social issues. As a group, these cohorts are delaying parenthood more than at any other time since the Great Depression (Rindfuss et al. forthcoming). In interpreting our results it is necessary to keep in mind the social-historical context in which these cohorts are becoming adults. Until parallel analyses are available from other cohorts, caution should be exercised in generalizing from our results.

For October 1 of each year, respondents were asked about a series of activities, all of which are preparation for the work sphere. (Data for 1975 were obtained retrospectively in 1976, and data for 1977 and 1978 in 1979.) After initial experimentation with alternative coding procedures, and to keep the number of activities manageable, we have classified each respondent's activity on October 1 of each year into one of five categories: work (W); school (E); homemaker (H); military (M); and other (N).<sup>3</sup>

<sup>2</sup> The NLS is a highly stratified sample, aiming to overrepresent students in high schools in low-income areas and with a high proportion of minority students. Other stratification variables include region, size of high school, proximity to colleges, community size, and high school type (private or public). This stratification, combined with the longitudinal nature of the data, gives rise to a very complicated weighting scheme. In our multivariate analysis, we ran models including and excluding these stratification variables. The results for the variables of substantive interest were unaffected by the inclusion of these stratification variables. Because of these results and Bruce Eckland's findings (personal communication) that weighting did not affect the distribution over college attendance categories, we present results from unweighted data.

See Riccobono (1981) for detailed documentation of the NLS data. For the analyses of this paper, we created a number of working files from the NLS. For information on these files, contact Erika Stone, Carolina Population Center, University of North Carolina, CB# 8120, University Square East 300A, Chapel Hill, NC 27514.

<sup>3</sup> Those with missing data are assigned to a sixth category in the multivariate analysis, but are excluded from the descriptive statistics. We do not, therefore, delete cases for people who skipped a follow-up or failed

<sup>1</sup> Looking at the effects of nonfamily activities on parenthood does not mean that we think the causality goes only one way, that the transition to parenthood has no effects on work and school activities. There is, indeed, a substantial literature suggesting that activities in the family sphere affect work and school activities. We do assume that behavior in the family sphere that comes before behavior in the work-school sphere is also causally subsequent. One might argue that *plans* for becoming a parent affect work and school activities. There is, however, considerable evidence that fertility expectations are poor predictors of fertility behavior and that changes in these expectations are themselves a function of nonfamily events.

"Other" includes looking for work, "hanging out", traveling, and various other miscellaneous pursuits. Despite this diversity, all the activities in the "other" category share the characteristic of being generally ignored by conventional life-course analysis. So few males reported homemaker as an activity that we included it for females only. Respondents can hold more than one activity state simultaneously, such as an individual who is going to school part time and working part time. In the case of jointly held activity states, we gave greater weight to military, schooling, work, and homemaker, in that order.<sup>4</sup>

Even though we have extremely rich data on the respondents' activities, *complete* life histories are not available for the years examined. The activity on October 1 may represent an activity of only a few days' duration on either side of October 1. When we examine sequences of these activities, what appears as an orderly sequence may not be, and vice versa. However, the original choice of October 1 as the reference month should minimize such problems. For the ages considered, the summer months are when individuals are most likely to be in temporary activities: summer jobs, a refresher course at school, or simply "hanging out." By October 1, those who are students would be in school. The job market would have solidified such that those who are unemployed then will probably spend more than just a few weeks in that state. And those who finished school the previous spring with the intention of joining the military (the draft is not a major concern for this time period) would likely be in the military by October 1.

There are several additional limitations to the data. Those who dropped out of school prior to senior year in high school, about 9 percent of this cohort (Bogue 1985, pp. 396-97) are not included. This is unfortunate. We expect that high school dropouts are more likely to experience disorderly sequences in both the work and family spheres. Also, at the time of the most recent follow-up, respondents were

aged 25 to 26. Although many of those who will become parents have done so by the age of 26, there are some who will become parents for the first time in their late 20s and early 30s (Rindfuss et al., forthcoming). Similarly, not all persons will have finished making the transition to an adult work role. The restriction to the mid-20s is not a major limitation as long as it is kept in mind that we are examining the life course of individuals from approximately age 18 through 25. The 1986 follow-up will allow us to subsequently extend the analysis through the early 30s.

The analysis is restricted to white, non-Hispanic respondents because we expect the structure of the process to be somewhat different for blacks and Hispanics. We will investigate the process for these other groups in subsequent work.

Also, all of our analyses are done separately by sex. Various states are likely to have different meanings, patterns, and effects for men and women (Marini 1984b, 1987). For example, being in the military is relatively rare for females, and, as noted above, the homemaker role is not an option that men indicate. Because of this latter pattern, males without jobs might state that they are unemployed, and females might respond that they are homemakers.

### *Disorder and Diversity*

Because the "normative" pattern is to complete school and then begin work, we classify the following sequences as consistent, i.e., expected orderly transitions: (1) Worked the entire eight years; (2) In school the entire eight years; and (3) School followed by work. Treating time in the military as a form of work adds: (4) In the military the entire eight years; (5) School followed by military; and (6) School followed by military followed by work. Because taking time out from school to "fulfill one's military duty" is considered conventional in some contexts, we include as well: (7) Military followed by school; and (8) Military followed by school followed by work.

Being a homemaker is also a form of work. Many, but not all, of the women who move in and out of the homemaker role do so in connection with parenthood. In fact, some might argue that an absence of several years from the labor force to have one or more children is "normal." Yet when viewed from the work sphere, moving in and out of the homemaker role introduces a discontinuity. This has long been discussed in the sex differences literature as one factor contributing to income disparities and occupational differences between women and men (e.g., Corcoran and Duncan

to answer the activity state questions. Note that these activity states do not assume any family role. In particular, it is possible for a woman to be a homemaker, but be neither a wife nor a mother.

<sup>4</sup> This procedure is based on the assumption that the most inclusive activity state should be given priority. It also makes the state space manageable. Considering various combinations of activities quickly increases the number and reduces the interpretability of states (see Marini 1987). Jointly held activity states (experienced by about 20 percent of the sample in any given year) are of interest in their own right, including as an additional form of inconsistency or disorder. From the perspective of examining inconsistency, our procedure is conservative in that we may be missing this disorder. We plan to examine multiple activity holding in future research.

Table 1. Percentage Distribution of Consistency of Activity States in Years 2 through 8 Following High School for White Males and Females

	Years Following High School						
	2	3	4	5	6	7	8
<i>Males</i>							
Apparently consistent <sup>a</sup>	84	76	69	62	59	55	51
Interrupt school	6	11	17	21	25	29	32
In and out of homemaker	—	—	—	—	—	—	—
In "other" category	8	10	11	13	13	13	13
Residual <sup>b</sup>	2	3	3	4	3	3	3
Total	100	100	100	100	100	100	100
<i>N</i>	8,176	7,740	7,300	7,300	6,691	6,691	6,696
Number of sequences	20	62	172	370	587	834	1,098
<i>Females</i>							
Apparently consistent	82	74	66	59	52	47	40
Interrupt school	5	10	15	19	23	26	31
In and out of homemaker	2	4	7	10	13	16	18
In "other" category	11	12	12	12	12	11	11
Residual	—	—	—	—	—	—	—
Total	100	100	100	100	100	100	100
<i>N</i>	8,174	7,871	7,526	7,526	7,095	7,095	7,095
Number of sequences	22	81	242	530	902	1,327	1,827

<sup>a</sup> See text for definitions.<sup>b</sup> Excludes those with missing data.

1979; Polachek 1975; Weiss and Gronau 1981; Wolf and Rosenfeld 1978; though see also papers in Reskin 1984). We therefore define as "orderly" sequences ending in homemaker, but those involving a move in and out of this role as inconsistent. The following, then, are consistent sequences: (9) Homemaker the entire eight years; (10) School followed by homemaker; (11) School followed by military followed by homemaker<sup>5</sup>; (12) School followed by work followed by homemaker; and (13) Work followed by homemaker.

The "inconsistent" sequences are: (14) In and out of homemaker, i.e., moving to another activity state after having been a homemaker; (15) Interrupted schooling, i.e., returned to school after having occupied one or more of the activity states (other than military); (16) In "other" category, i.e., spending one or more years in the "other" category; and (17) Residual, i.e., any sequence that does not fall into one of the other sequences. Empirically, these all involve having been in the military at an unconventional time.

When a sequence involved a mix of states, different times spent in the various categories were considered equivalent in terms of consistency or inconsistency. For example, the sequence EEEWWWW would be placed in category (3), as would EWWWWWW.

To some extent, the classification of sequences into "consistent" and "inconsistent" is arbitrary. This exercise shows, if nothing else,

that when one considers a somewhat expanded set of activities over time, the idea of a "normal" sequence is not always clear-cut. However, these classifications do reflect discussions in the life-course and work-life literature. Within the set of consistent and inconsistent sequences, some will be more likely than others, and some will be more favorably looked upon by parents and advisers than others. For now, we treat the 13 variations of consistent categories the same and the 4 inconsistent sequences the same. In later sections we look directly at the sequence of activities rather than using this classification.

If we assume each event has an equal probability in each of the eight years, then there are  $5^8$ , or 390,625 possible sequences. To the extent that life-course transitions are orderly and irreversible, we would expect respondents to fall into a relatively small number of these possible combinations, and the overwhelming majority to fall into the consistent sequences. As is apparent from the last column in Table 1, this is not the case. After eight years, only a bare majority of the males and an even smaller proportion of females follow a consistent sequence. Further, given the number of respondents, the number of sequences found is quite large. By the end of eight years, it takes 1,100 sequences to describe the experiences of 6,700 young men and 1,800 for 7,000 young women. On average, there are only six men in each male sequence, and only four women in each female sequence. Furthermore, there are numerous sequences with only one or two individuals.

As might be expected, the most frequent single sequence is working all eight years

<sup>5</sup> School followed by military followed by work followed by homemaker is a logical possibility, but there is no one in this category.

post-high school. Among the males, there are 16 percent in this category. After this category, the seven possible sequences of school followed by work are quite common. Treating the seven as a single group, 28 percent of the males and 21 percent of the females follow these sequences.

Leaving school is clearly quite reversible. Even without considering breaks for military service, during the first eight years after high school almost a third of the men and women return to school at least once after having left for one or more years.<sup>6</sup> Similarly, spending one or more years in the "other" category is far from rare. Finally, almost one of every five females spends some time in the homemaker status and then moves into some other status—a possibility typically not considered in the empirical life-cycle literature, although it is treated in the female labor-force participation literature. Indeed, moving in and out of the homemaker status is responsible for the large difference in the proportion of males and females in the consistent category.

To consider the process underlying these sequences, we examined the proportion consistent in each of the years 2 through 8. Right after high school, the probability of being in an inconsistent sequence is low for the simple and mechanical reason that there has been relatively little time to be so, as can be seen in Table 1. Past the second year, however, the proportion with consistent sequences decreases steadily for both men and women, and at a more rapid pace for women. No one particular year accounts for the large proportion of apparently inconsistent sequences at the end of eight years post-high school. Rather, there is a steady growth in the number that move in and out of the homemaker category, interrupt their schooling, or spend one or more years in the "other" category.

The implications of these results for life-course analyses are clear: we cannot assume that life course transitions are either orderly or irreversible, and the patterns are considerably more complex than is reflected in previous research. However, the implications for analysis beyond description are not yet clear. If the process tends to forget its past, then the apparent disorderliness of early adult years would be of no further consequence. But if it does not forget its past, then we need to bring disorder explicitly into our analytical models. To what extent do inconsistent sequences matter? We now turn to the question of whether and how much diversity and disorder in the nonfamilial sphere affect the timing of the transition to

parenthood, after we describe how we set up our analysis.

### *The Transition to Parenthood: Specification*

Our analytical approach examines a series of conditional first-birth probabilities. The universe consists of those who are still not yet parents at time  $t$ , i.e., only those at risk of becoming a parent for the first time. For example, the less than 10 percent of the sample who experienced first births before 1974 (the first year when we can calculate a sequence effect) are excluded from all our later analyses. The dependent variable is a dichotomy: whether the respondent (not yet a parent) becomes a parent during the year. We use logistic regression to estimate models. This design is essentially that of a multivariate life table. Because yearly estimates are being produced, we can allow individuals to change activity states from year to year. We can also incorporate complex sequences of activity states into the analysis.

If only the most recent (i.e.,  $t-1$ ) activity has an effect on the transition to parenthood, then we will conclude that the process' earlier history is unimportant. But if earlier (i.e.,  $t-2$  and  $t-3$ ) activity states are also important predictors and if their sequence matters, then we will conclude that researchers need to take into account more distant history. We not only use statistical criteria of improvement in models' fit to interpret our results, but also look at the pattern of activity states' effects to see whether their pattern gives us a sense of the results' substantive meaning.

Our approach preserves the temporal ordering of the activity states relative to the timing of parenthood and is suggestive of causality. Yet temporal ordering need not be equivalent to causal ordering. Individuals may anticipate becoming parents several years in the future and alter their current activity state in preparation for parenthood. Under such circumstances, the causal flow would be from fertility to activity state. While we cannot dismiss this hypothetical reverse causality, we have no firm empirical or theoretical evidence suggesting it may be present (as discussed in note 1).

We have also tried to minimize this possible causality problem in our operationalization of the transition to parenthood. The 1979 round of the NLS72 study collected a complete fertility history. Thus, unlike the activity states, all respondents can be classified monthly as to whether they have become parents. This provides us with more flexibility in specifying our parenthood variables than with the activity states. We examine fertility in terms of parental status for a given 12-month interval, but this could be any given 12-month interval. The exact

<sup>6</sup> If we were not forced by the nature of the data set to exclude high school dropouts, the proportion returning to school would certainly be even higher.

starting point for the interval is arbitrary. For example, a couple may decide to have a child and to deliberately stop using contraception. Assuming they were not using the pill, the average waiting time to conception is five to seven months, and the average length of gestation is nine months. Thus, the couple might expect to have a live birth 14 to 16 months after the activity state. If the couple had been using the pill, the waiting time to conception might be several months longer. If the couple did not deliberately stop but, rather, relaxed the vigilance with which they used contraception, the waiting time to conception would be longer still.

Conversely, the measurement of activity states is fixed as of October of each year. However, the effect of that activity state or set of prior states might have begun many months prior to October. This is particularly problematic when there is a change in activity from one year to the next. For example, a change from being a student to being a full-time worker will typically take place in June rather than October. Furthermore, such changes are usually anticipated for at least several months. To continue with the student-to-worker example, a student might be able to accurately predict the change in activity status and plan accordingly. Thus, an October activity state might actually have influenced a decision to have a birth that occurred in the preceding month.

These situations obviously imply different lags between the activity state and the first birth. To balance these considerations, we examine the effects of activity states in October of a given year (and prior years) on the probability of becoming a parent for the first time 6 to 17 months after that October (e.g., the effect of activity state in October 1974 on the probability of having a first birth between April 1975 and March 1976). This definition of our dependent variable allows for continuity and anticipation effects for the activity states as well as sufficient time for conception and gestation following the October measurement. Alternative specifications of the parental status variable that moved the 12-month fertility interval several months closer to and farther from the activity state produced substantially similar results.

In addition to activity states, we controlled for several variables many fertility studies have found important: farm background, religion, region, and parental socioeconomic status (SES), which is based on a composite measure that included information on mother's education, mother's occupation, parent's income, father's occupation, and durable consumer items in the household (Riccobono et al. 1981, Appendix K). While these variables are included as controls in the present analyses, we will not show or discuss their effects here, since such

effects have been extensively studied elsewhere. All the background variables had the expected effects.

We leave the marital status of the respondent out of our initial analysis of the fertility data for several theoretical and empirical reasons. First, as we will discuss in more detail later, we do not have unambiguous marriage histories for some respondents. Unlike fertility, complete marriage histories were not obtained in 1979. Second, even if we did have complete and unambiguous marriage histories, marriage should not be included as a predictor variable on theoretical grounds (Hirschman and Rindfuss 1982). Parenthood often precedes marriage, thus making it difficult to argue that the causal direction runs from marriage to fertility. A child born before marriage may significantly delay the unwed mother's first marriage. Parenthood preceded marriage for about 10 percent of white women in the 1970s. On the other hand, premarital conceptions may hasten a marriage or lead to a marriage that otherwise would not have taken place, in order to legitimate the ensuing birth. Approximately 40 percent of premarital pregnancies resulting in births were legitimated in the 1970s (Jones et al. 1985), again suggesting that the causal relationship between the transitions to marriage and to parenthood is not unambiguous. Furthermore, even if parenthood does not precede marriage, for many couples it is a joint decision—that is, the couple decides about marriage and parenthood as if it were a single decision. There is strong indirect evidence that such joint decisions are typical in Japan (Morgan et al. 1984). While the decisions about marriage and parenthood are not joint for all couples in the United States, if they were joint for even a significant minority, it would be clearly inappropriate to have either one predicting the other. For these reasons, the main part of our parenthood analysis excludes marriage. After presenting the main part of our results, we examine the effects of activity sequences when marital status is controlled. This, in essence, assigns causal priority to marriage, which is the typical, if erroneous, assumption in much of the literature.

#### *Inconsistency and the Transition to Parenthood*

The first question we asked is "Does having experienced an inconsistent sequence per se have any effect on the timing of the transition to parenthood?" One might argue that, other things being equal, experiencing an inconsistent sequence would make an individual hesitant to become a parent, postponing the transition. This, of course, brackets all inconsistent sequences together and suggests that inconsistency has a unified meaning. The advantage of this

Table 2. Effect of Having a Consistent Life-Course Sequence on the Log-Odds of Making the Transition to Parenthood

	Having a Child in:				
	1974	1975	1976	1977	1978
<i>Females</i>					
Coefficient	-0.51**	-0.06	-0.15	-0.33**	-0.29*
N	6,256	5,824	5,369	4,956	4,597
Observed Probability	.06	.07	.08	.07	.07
<i>Males</i>					
Coefficient	-0.01	-0.01	0.27	-0.04	0.24
N	6,266	5,953	5,608	5,316	5,027
Observed Probability	.04	.05	.05	.05	.06

Note: Background variables and  $t-1$  activity states are also included.

formulation is its parsimony. Its disadvantage will become evident later. To examine this inconsistency question, a dummy variable was built indicating whether the individual had experienced an inconsistent sequence, with inconsistency defined as previously. It is coded 1 if the person had a consistent life-course sequence, 0 otherwise. This variable refers to consistency or inconsistency prior to the year in question and thus can change in subsequent years. But by our definition, once people become inconsistent, they stay inconsistent.

Table 2 shows the effects of consistency/inconsistency on the log-odds of making the transition to parenthood in a given year net of background variables and the previous year's activity. Overall, the effect of having experienced a consistent versus an inconsistent sequence is not substantial. In no case is the effect significant for males. For females, those who have experienced a consistent sequence are less likely to become mothers than those who experienced an inconsistent one. In three of the five years that we examine, this effect is statistically significant.

The pattern of results shown in Table 2 could be caused either by inconsistency not having much of an effect on the timing of parenthood or by the existence of heterogeneous effects within the inconsistent category. The latter possibility cannot be examined by simply entering a single dummy variable for consistency versus inconsistency.

We next examine a series of models that included pairs of adjacent activity states. We examined the effects of the prior two years in two different ways: an additive model and an interactive model. In the additive model, we included activity states from years  $t-1$  and  $t-2$  to try to predict fertility in year  $t$ . This answers the simple question of whether the process forgets its past. If it does, then only the coefficients for  $t-1$  activity states should be significant. Thus, the additive model asks whether knowing what the person did in year

$t-2$  statistically improves our prediction over and above just knowing what the person did in year  $t-1$ . However, the presence of significant  $t-2$  effects on the timing of parenthood does not necessarily mean that disorder or inconsistency is important. To see whether the actual sequence of activities matters, we built variables for all possible cross-classifications of activity states in years  $t-1$  and  $t-2$ . Any time there were 25 cases or more in a cell, that variable was included in the interactive model. If there were less than 25 in a cell, they were put in a residual category. The interactive model asks the further question of whether the effect of one's activity status in year  $t-1$  depends on one's activity state in year  $t-2$ . While the additive model is the more parsimonious, the interactive model makes the effects of different combinations visible and lets us see whether sequences implying inconsistency make a difference.

The overall comparison of the two-year and single-year models is shown in Table 3.<sup>7</sup> In general, for both males and females the additive two-year activity state models fit better than the one-year activity state models. The process does not forget its past. Note that this is not true for every single year. The last panel of Table 3 compares the two-year interactive to the two-year additive model. Although there are exceptions, the two-year interactive model is not generally a significantly better fit than the additive model. The reason, as will become evident below, is that several of the interactive sequences have similar effects.<sup>8</sup> We collapse these in later analysis.

We now focus on the full interactive models to highlight the effects of the diverse combinations. Since we have, on average, fifteen activity state combinations times five years

<sup>7</sup> These models all include the set of background variables described earlier.

<sup>8</sup> In essence, degrees of freedom were being used unnecessarily for sequences that have similar effects.



Table 3. Comparison of Selected Two-Year Activity State Models to One-Year Additive Models: Likelihood Ratio Chi-Squares (d.f.)

	1974	1975	1976	1977	1978
<i>Single-year model</i>					
Females	369.2 (9)	336.6 (9)	264.4 (9)	131.6 (9)	90.6 (8)
Males	142.4 (9)	233.7 (9)	183.2 (9)	83.0 (9)	65.7 (8)
<i>Two-year additive model</i>					
Females	410.7 (13)**	376.4 (13)**	275.7 (13)*	163.3 (12)**	108.7 (12)**
Males	154.9 (13)*	236.0 (13)	195.1 (13)*	104.2 (12)**	84.9 (12)**
<i>Two-year interactive model</i>					
Females	429.3 (20)**	408.9 (21)**	286.2 (18)	177.1 (19)	116.7 (18)
Males	169.6 (18)*	250.7 (21)	194.5 (19)	110.7 (18)	87.1 (17)

\* Significant increase in  $\chi^2$  over previous model at .05 level.\*\* Significant increase in  $\chi^2$  over previous model at .01 level.

times two sexes, we decided against showing all the coefficients. Instead, Figures 1 through 4 graphically display sets of effects on log-odds for males and females. In all cases, the reference group is those having worked in both year  $t-1$  and  $t-2$  and is indicated by the zero line on the graph. Since the stability of these coefficients is dependent on the number of cases in the combination, we have graphed the coefficient plus and minus one standard error.

Figure 1 contrasts the four different combinations of having worked in year  $t-1$  and one of four activities in  $t-2$ . Figure 1 makes it clear why our consistent-inconsistent variable was not very powerful: different types of inconsistency have different effects. By our definition, both homemaker followed by work and other followed by work are inconsistent. Yet, those who have been in the homemaker-work sequence are more likely than those with work-work to make the transition to parenthood at time  $t$ , and those who have been in the other-work sequence tend to be less likely to make the transition to parenthood. This difference also makes reasonable post hoc sense. Those in the other-work sequence probably want to solidify their job status before they consider parenthood. Further note that the effects of school-work and other-work are quite similar for both males and females. Yet, school-work is an orderly sequence and other-work is a disorderly one. Thus, inconsistency can have the same effect as consistency. Again, it appears that the struggle to obtain a job, whether through education or having been unemployed, lessens the likelihood of becoming a parent relative to the individual working two years in a row.

Those of either sex who have been in school two years in a row are less likely to become parents than those who were in school in  $t-1$  but doing something else in  $t-2$ . This is shown

in Figure 2, where, again, the reference category is working in both years. Thus, those who have returned to school may be less seriously committed to their education than those who have stayed in for at least two years. Further, among those not in school in  $t-2$ , it seems not to matter what they were doing then.

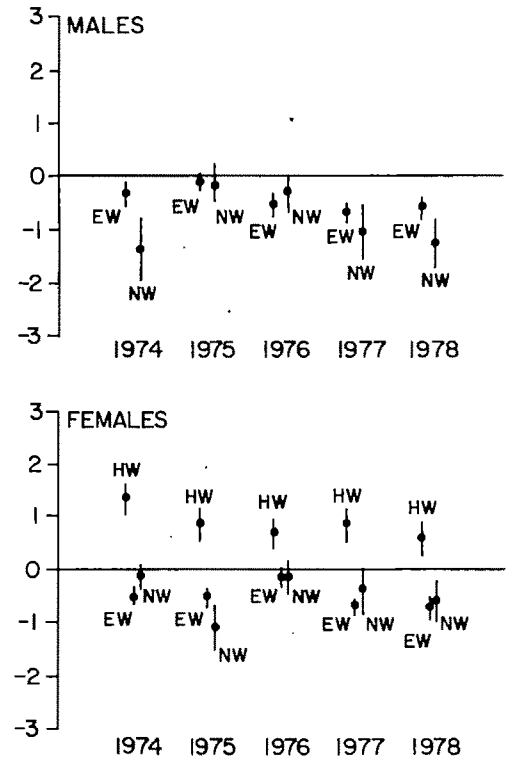


Fig. 1. Betas (plus or minus one standard error) from a Series of Conditional Logistic Regressions Predicting the Transition to Parenthood in the Indicated Year: Activity State Combinations Ending with Work, 1974-1978, Males and Females

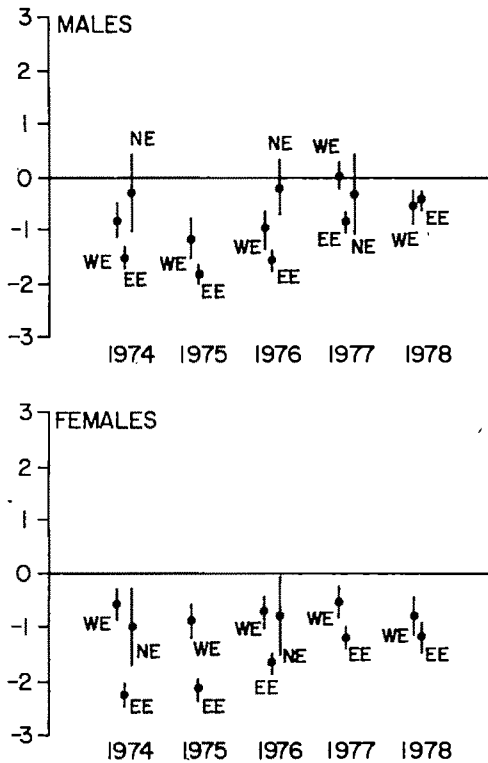


Fig. 2. Betas (plus or minus one standard error) from a Series of Conditional Logistic Regressions Predicting the Transition to Parenthood in the Indicated Year: Activity State Combinations Ending with School

Figure 3 contrasts the three different combinations of other in year  $t-1$ . Although there are some exceptions, if people were unemployed or "hanging out" in year  $t-1$ , then it does not matter what they were doing in year  $t-2$ . Further, in 1976 to 1978, being in the "other" category tends to suppress the probability of becoming a parent.

Finally, for females, Figure 4 shows the four different combinations with homemaker for  $t-1$ . As was the case with "other," in general it does not matter what women were doing in year  $t-2$  if they were homemakers in year  $t-1$ .

To summarize, these results provide many examples of where the process does not forget its past. Further, they suggest that there are heterogeneous inconsistency effects. The question that naturally arises now is, after controlling for activity states in  $t-1$  and  $t-2$ , does activity state in  $t-3$  matter? Put differently, for how long does the process remember its past? To examine this question, we simplified the two-year interactive model along the lines suggested above, combining sequences that are substantively and empirically similar. For example, we found that military service for men could be treated the same as work in its effect on

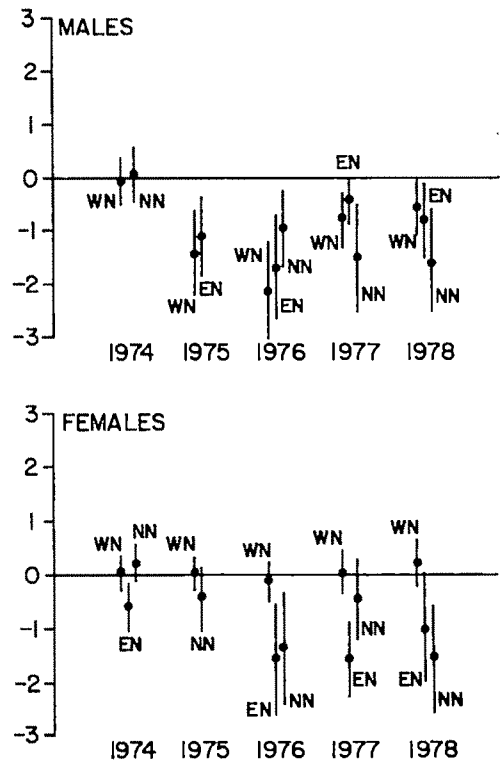


Fig. 3. Betas (plus or minus one standard error) from a Series of Conditional Logistic Regressions Predicting the Transition to Parenthood in the Indicated Year: Activity State Combinations Ending with Other

parenthood. Then we added activity states from  $t-3$ .<sup>9</sup> Again we constructed two different types of models: additively including  $t-3$  activity states and interacting  $t-3$  with the  $t-1/t-2$  combinations.

In general, as Table 4 shows, including  $t-3$  activity states in the model additively again shows that it is important to consider the history of nonfamilial events in predicting the transition to parenthood. Out of the four years for which we can include three previous years' activities, we find  $t-3$  activities significantly improve the fit of the model, even after including activities of  $t-1$  and  $t-2$  in three years for women and two years for men.

This occurs even though it is difficult for activity state  $t-3$  to affect the conditional probability of becoming a parent at time  $t$ . First, remember that the dependent variable is a probability and only includes those at risk.

<sup>9</sup> The retained sequences for females are homemaker ( $t-1$ ), homemaker-work, work-work, school-work, other-work, work or other-school, school-school, work-other, school or other-other. Despite its substantive interest, the case base for female military service was too slim to support empirical analysis.

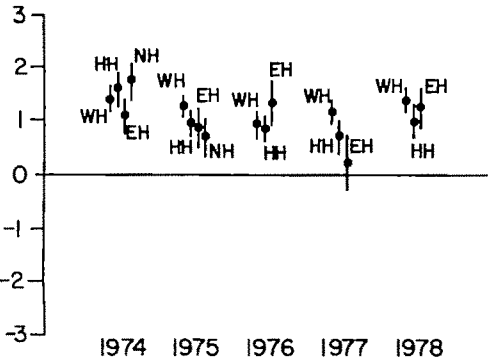


Fig. 4. Betas (plus or minus one standard error) from a Series of Conditional Logistic Regressions for Females Predicting the Transition to Parenthood in the Indicated Year: Activity State Combinations Ending with Homemaker

Those who became a parent at an earlier time are excluded. Further, remember that we have shown that activity state at  $t-3$  affects the conditional probability of becoming a parent at  $t-2$  and  $t-1$ . Thus, the universe at  $t$  is already partly shaped by the effect of the  $t-3$  activity state. So when we ask if activity state at  $t-3$  influences the conditional probability of becoming a parent at  $t$ , the sample is already selected based on its  $t-1$  and  $t-2$  influences. Also note that the tests of the statistical significance of adding the  $t-3$  activity states to the two-year model are global tests for the inclusion of all dummy variables for the  $t-3$  activity states. In every instance where the global tests are insignificant, one or more of the individual coefficients are statistically significant. In all four years, for both males and females, the probability of becoming a parent is less for those who were in school three years prior compared to their working counterparts. Similar reduced first-birth probabilities were found for males and females in the "other" category for most intervals, indicating the persisting effects of

unemployment or other disengagement from more routine activities.

To continue our investigation of the effects of inconsistency, we constructed interaction terms between the  $t-3$  activity states and the two-year sequences discussed above where a sufficient number of cases was available. By and large, these three-year interaction models did not provide a better fit than the additive models. This is not surprising, because some of the detail in the latter models is lost because there are too few cases to construct a three-year sequence term. Nevertheless, the full interactive model made some interesting inconsistency results visible, and we have included two examples in Figure 5.

The three-year interaction models contrast the effects of various sequences of activity states with those for men and women who were working all three years prior to the interval under consideration. The bottom panel of Figure 5 shows the logistic regression coefficients (plus and minus their standard error) for women who were homemakers at  $t-3$  and working the two following years (HWW) with women in school at  $t-3$  and also working the next two years (EWW). The former is an inconsistent sequence, while the latter is a consistent one. At all four times, women who were homemakers at  $t-3$  were more likely to become mothers during the interval than women in school at  $t-3$ . Again the evidence indicates that certain inconsistent sequences promote parenthood while others inhibit it.

The top panel of Figure 5 compares an inconsistent three-year sequence for men with a consistent one. The coefficients are for men who were working at  $t-3$  and in school on October 1 of the next two years (WEE), contrasted with those for men attending school all three years (EEE). As anticipated, men consistently attending school have lower birth probabilities. What is more surprising is that, except for 1975, the men who worked at  $t-3$ , but were in school the next two years, were not significantly more

Table 4. Comparison of Collapsed Two-Year Interactive Model with Three-Year Model: Likelihood Ratio Chi-Squares (d.f.)

	1974	1975	1976	1977	1978
<i>Two-year interactive model</i>					
Females	—	390.1 (14)	286.9 (14)	168.1 (14)	118.5 (14)
Males	—	236.3 (13)	190.2 (12)	107.0 (12)	83.8 (12)
<i>Three-year model</i>					
Females	—	404.1 (18)**	296.5 (18)*	171.8 (18)	148.8 (18)**
Males	—	244.9 (17)	207.3 (16)**	114.2 (16)	96.3 (16)*

\* Significant increase in  $\chi^2$  over previous model at .05 level.  
\*\* Significant increase in  $\chi^2$  over previous model at .01 level.

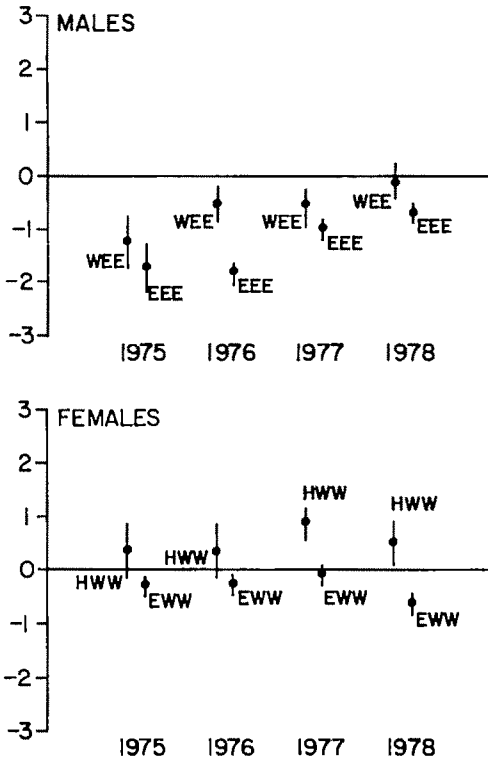


Fig. 5. Betas (plus or minus one standard error) from a Series of Conditional Logistic Regressions Predicting the Transition to Parenthood in the Indicated Year: Selected Activity State Combinations

likely to become parents than were men working all three years prior to the interval. Compared with the corresponding pattern of coefficients (not shown) for women, it appears that the negative effects of education on the transition to parenthood are more pronounced for women than for men. More generally, the results indicate that the consequences of the same life-course sequence may be quite different when experience by men compared with women.

### *The Question of Marriage*

So far, the parenthood analysis has not explicitly considered marital status. The reasons outlined earlier are both theoretical and empirical: Yet, one might object that, indirectly, marital status is being controlled through the homemaker activity status variable. Although the correlation is far from perfect, empirically it is unusual for a woman to be a homemaker unless she is currently married.

Reflecting the differences in familial roles for men and women in the contemporary United States, men simply do not report themselves as being in the homemaker activity state. Thus, our results for males are less affected by the issue of

marriage, and the basic finding of the previous section is still valid, namely, that it is more than the last activity state that matters. In short, we need to take into account the sequence of activity states. However, because of the connection between homemaker and marital status, this conclusion for women is a bit more uncertain.

To further explore this question, we constructed a marital status variable for October of each year from 1972 to 1976 (1977 and 1978 are not used because we lag marital status two years behind the fertility variable). Unfortunately, a complete marriage history is not available; the problem is that we do not know marital dissolution dates.<sup>10</sup>

As noted earlier in this paper, one of the main problems with trying to include marital status when modeling the first birth process is that, for some individuals, the decision to marry and the decision to become a parent are the same decision. To the extent that this is the case, treating marriage as a predictor variable is the equivalent of taking a component of the dependent variable and making it an independent variable. This is clearly unacceptable because not only will it lead to a strong, but causally erroneous effect of marriage, but it will also tend to weaken the effects of the other variables in the model. However, we want to examine the robustness of the previously presented disorder effects. If we include marital status as a predictor variable and still find evidence that order is important, then we will have increased confidence in the finding. But, since we have no knowledge of the extent to which marriage and fertility decisions are joint, we do not know the extent to which such a model underestimates order effects.

To explore this general issue, we re-estimated the two-year interactive models for females, including marital status at time  $t-2$  as a control variable for each year 1974 through 1978. The results are summarized in Table 5 for two sets of

<sup>10</sup> In each of the first three follow-ups, a question on marital status was asked for the first week of October of that year. Thus, for October 1973, 1974, and 1976, we can classify individuals unambiguously by marital status. For October 1972, we use the question from the fall 1973 follow-up, which asked for the date of first marriage. For those with an uncomplicated marital history, this results in the correct classification. However, some individuals with a complicated marriage history may be incorrectly classified. For October 1975, we have a similar problem. The fall 1976 interview asked about the date of the respondent's marriage without reference to the order of the marriage. Again, for those with an uncomplicated marriage history, our classification will be correct; but some of those with a complicated marriage history may be incorrectly classified. Fortunately, this soon after high school graduation, relatively few women will have had a complicated marriage history.

Table 5. Effects of Indicated Combinations of Adjacent Activity States and Marital Status on the Log-Odds of the Transition to Parenthood, Females

Variable	1974	1975	1976	1977	1978
<i>Activity state combinations<sup>a</sup></i>					
Homemaker-work	0.82*	0.35	0.12	0.41	0.05
School-work	-0.47*	-0.34	0.09	-0.30	-0.45*
Other-work	-0.13	-0.97*	0.13	0.05	-0.24
Work or other-school	-0.55	-1.04*	-0.68*	-0.59	-0.78*
School-school	-2.17*	-1.95*	-1.24*	-0.75*	-0.87*
Marital status at year 2	0.85*	0.93*	1.35*	1.37*	1.56*

Note: The model also includes background characteristics and a residual set of activity state combinations not shown here.

<sup>a</sup> The omitted category for the activity state categories is work-work, and for the marital status variable is unmarried.

\* Indicates significance at the .05 level.

activity state combinations: those involving work at  $t-1$  and those involving school at  $t-1$ . First, as expected, the marital status effect is strong in each of the five years. It also increases in strength from 1974 to 1978, perhaps suggesting that marriage and parenthood decisions are more likely to be joint in one's mid-20s.

Table 5 also continues to show order effects, even though they are diminished by the inclusion of the marital status variables. Consider first the effects involving work at  $t-1$  and remember that the combination work-work is the reference category. In each of the five years, having been a homemaker at  $t-2$  increases the probability of becoming a parent at  $t$ . And in most of the five years, having been in school or the "other" category reduces the probability of becoming a parent. The effects involving those in school in  $t-1$  also indicate the importance of the activities' order. In each year, those who were in school the two previous years have a lower probability of making the transition to parenthood than those who were either working or unemployed at  $t-2$ . These patterns of order and disorder effects are the same as those found when marriage was not included as a control variable.

Despite our theoretical reservations about including marital status as a control variable, the results in Table 5 support the earlier ones, that the order of events in the nonfamily sphere affect the timing of the transition to parenthood. To the extent that marriage really is a separate precondition for fertility, then the results presented earlier may overstate the magnitude of the disorder effects for women. If, on the other hand, marriage and fertility are joint decisions, then the results in Table 5 understate the magnitude of the disorder effects. While resolving this issue is beyond the scope of this paper, either way, there appear to be order effects.

#### *Interrupted Schooling and Becoming a Parent*

Up to this point, we have followed the strategy of looking at all possible combinations. Even

collapsing sequences that seem to have similar effects on the transition to parenthood quickly becomes cumbersome and lacks theoretical elegance. Another approach is to focus on one type of activity, one that seems to be especially important, and examine how, over time, the patterns of that activity affect the transition to parenthood. Education is the best candidate. The work discussed in the previous section of this paper shows that the timing of education is crucial to the timing of the transition to parenthood. The educational patterns people could follow are continuing in school after high school, then leaving; staying in school without interruption; returning to school after some interruption (including interruptions for the military, given the focus here on school versus all other activities); and leaving high school without a return to school. Of course, these categories are with respect to a particular time after high school. Someone could be classified as being out of school since high school in year  $t-2$  and, with a return to school between year  $t-2$  and year  $t-1$ , as having interrupted schooling at  $t-1$ .

Table 6 shows the effects for the first three schooling patterns compared with the effect for those who did not attend school beyond high school. The results are consistent across years and sex, and they are intuitively appealing. Those who did not attend school since high school were the most likely to become parents; those who were in school continuously through  $t-1$  were the least likely. Returning to school has the next largest negative effect, followed by having been in school after high school for some years but having left to do something else. The last contrast, though, is not statistically significant for men. If becoming a parent were close to inevitable at the ages covered here, we might expect to see the effect of schooling patterns decline over time, but that is not generally the case.

Clearly, schooling patterns affect when one becomes a parent; but does this parsimonious model do the job of the more complicated and detailed specification of activity patterns? No.

Table 6. Effects of Schooling Patterns on the Log-Odds of Transitions to Parenthood

Schooling Pattern	1974	1975	1976	1977	1978
<i>Females</i>					
Interrupted schooling <sup>a</sup>	-0.82**	-0.74**	-0.55**	-0.49**	-0.52**
School, then something else	-0.52**	-0.31*	-0.31*	-0.45**	-0.60**
All school	-2.48**	-2.32**	-2.13**	-1.24**	-1.82**
$\chi^2$ (d.f. = 9)	322.90	297.93	255.11	101.24	50.47
Fit compared with two-year additive model	+	+	+	+	+
<i>Males</i>					
Interrupted schooling <sup>a</sup>	-0.69*	-0.78**	-0.91**	-0.24	-0.33*
Schooling, then something else	-0.27	-0.15	-0.25	-0.26	-0.21
All school	-1.41**	-1.83**	-1.67**	-0.92**	-0.79**
$\chi^2$ (d.f. = 9)	146.36	212.66	180.44	88.48	67.67
Fit compared with two-year additive model		+	+	+	+

Note: Background characteristics are also included in the model.

<sup>a</sup> Reference category = no school since high school.

\* .01 <  $p$  ≤ .05.

\*\*  $p$  ≤ .01.

+ Indicates the two-year additive model is a significantly better model than this schooling model at the .05 level.

In only one case is the schooling model as good or better a fit than the two-year additive model.

The analysis in Table 6 puts all of the burden of the activity states' effects on the schooling pattern variables. In the previous analyses, we included activity state at time  $t-1$  as one of the control variables. Does doing that here bring us to a model that is equal in predictive power to one with detailed activity histories? Table 7 shows the results of including schooling patterns from high school to  $t-2$  and activity state at  $t-1$ . The effects of the schooling patterns are weaker, but having stayed in school for some

time after high school still tends to consistently decrease the probability of becoming a parent. In three years, for both men and women, this model does as well as the model with the interaction of  $t-1$  and  $t-2$  activity states. However, it does as well as a model which includes the third previous year in only one case for women and two for men. The latter is the more telling comparison because the results in Table 5 use data on activity states for at least three years (at least two years to define schooling pattern and  $t-1$ ). In short, simply using consistency or inconsistency in the

Table 7. Effects of Schooling Patterns through  $t-2$  on Log-Odds of Transitions to Parenthood, Net of Background Variables and  $t-1$  Activity State

Schooling Pattern	1975	1976	1977	1978
<i>Females</i>				
Interrupted schooling <sup>a</sup>	.39	-.11	-.29	-.20
School, then something else	-.16	-.30*	-.07	-.47**
All school	-.88**	-.76**	-.95**	-1.16**
$\chi^2$ (d.f.)	368.72 (13)	286.72 (13)	166.78 (13)	123.57 (12)
Fit compared with two-year interactive model	+			
Fit compared with three-year interactive model	+		+	+
<i>Males</i>				
Interrupted schooling <sup>a</sup>	.18	-.59*	-.39*	-.20
School, then something else	-.16	-.37*	-.05	-.25
All school	-.33	-.72**	-.80**	-.39
$\chi^2$ (d.f.)	238.56 (13)	194.31 (13)	109.05 (13)	72.81 (12)
Fit compared with two-year interactive model				+
Fit compared with three-year interactive model		+		+

Note: Background characteristics are also included in the model.

<sup>a</sup> Reference category = no school since high school.

\* .01 <  $p$  ≤ .05.

\*\*  $p$  ≤ .01.

+ Indicates model is significantly better fit than schooling model.

educational sequences to simplify the modeling of the process is not sufficient.

## DISCUSSION AND CONCLUSIONS

We set out to examine the extent to which people's lives deviate from the model implicitly assumed by many of those who study the life course and whether deviations from this model made a difference in the timing of an important life-course transition, parenthood. We looked at the order of events in the nonfamily sphere and found that, by the time they had been out of high school eight years, over half the men and women in the 1972 high school class had sequences of activities inconsistent with what is often assumed to be the "normal" pattern. Some of this inconsistency involved reversing the process of leaving school, i.e., returning to school after some absence. But a significant amount involved movement in and out of activities not usually considered in life-course analysis: those of "other" and homemaker. A simple measure of inconsistency, though, did not uniformly affect when people made the transition to parenthood. This is not surprising, because there are heterogeneous effects of the activities that make up the inconsistent patterns. Further, while both the history of activities and the actual sequences in many cases affected when people first became parents, it was not always inconsistency that had the most dramatic effects.

We investigated the extent to which our findings were the result of omitting the marital status variable. Although we had not included this variable in our models for both empirical and theoretical reasons, some could argue that the effects of especially the homemaker variable are just proxies for marital status. Including marital status decreased, but did not erase, the effects of activities' history and sequence. We also tried to simplify our models by following patterns of an extremely important activity, schooling. Again, consistent patterns of schooling, especially continuing in school after high school without interruption, had more striking effects on entering parenthood than inconsistent patterns. However, following only education did not, in general, give predictive power quite as strong as when using the more complicated models. We feel our conclusions are robust.

Readers will have noticed that we did not hypothesize how we expected sequences of activities to affect the transition to parenthood, beyond the one implied by the concept of "disorder" or "inconsistency" (embedded in the few attempts to look at effects of such sequences)—that inconsistency is a deterrent to making a major role change. We have simply complicated the study of the life course in this paper. The next task is to simplify it again by

determining what underlies the effects we have discovered. Certainly, extant theories of fertility or other dependent variables of interest can explain some of the effects. For example, one can argue that staying in school past high school represents a period of limited means when having a child could strain both financial and time resources. The "other" activities category could also be a proxy for a time without a stable income. Sørensen's work on the effect of income stability on the transition to marriage among Norwegian men shows the importance of this factor. The level and stability of economic resources might be one place to start looking for why we found the effects that we did.

On the other hand, incumbency in any role/activity state will be associated with influences rooted in factors other than economic resources. One important dimension here may be the commitment to the role itself (Clausen 1972; Modell et al. 1976). This dimension may help to explain why some past roles affect the timing of behavior, while others do not. For example, we find strong effects of the homemaker role even if the role was experienced in the past and was subsequently replaced by worker status. These effects may reflect commitment to traditional sex roles as well as the economic and social opportunities for becoming a parent. A similar interpretation may be advanced concerning how the continuous occupancy of a role, such as student, results in cumulative effects.

These considerations suggest that understanding the nature and importance of sequencing in the life course requires analyzing what the roles themselves mean and how they are causally linked. The reversibility and diversity of the nonfamilial domain for the cohorts we have examined further suggest that identifying the age of first entry into various adult roles and the order in which these entries occur will provide an incomplete picture of the process. One strength of our analytical approach is to identify what people are doing each year of late adolescence and early adulthood, as opposed to identifying the age at which arbitrary transitions occur. We hope this paper does not stimulate more work on the determinants of first birth, first marriage, or educational attainment, but, rather, encourages a more careful look at the life course *as it is actually lived*, not as we wish it to be for the sake of order in research.

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# TRENDS IN THE RESIDENTIAL SEGREGATION OF BLACKS, HISPANICS, AND ASIANS: 1970-1980\*

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*This paper examines trends in residential segregation for blacks, Hispanics, and Asians in 60 SMSAs between 1970 and 1980 using data taken from the 1970 Fourth Count Summary tapes and the 1980 Summary Tape File 4. Segregation was measured using dissimilarity and exposure indices. Black segregation from Anglos declined in some smaller SMSAs in the south and west, but in large urban areas in the northeastern and north central states there was little change; in these areas blacks remained spatially isolated and highly segregated. The level of black-Anglo segregation was not strongly related to socioeconomic status or level of suburbanization. Hispanic segregation was markedly below that of blacks, but increased substantially in some urban areas that experienced Hispanic immigration and population growth over the decade. The level of Hispanic segregation was highly related to indicators of socioeconomic status, acculturation, and suburbanization. Asian segregation was everywhere quite low. During the 1970s the spatial isolation of Asians increased slightly, while dissimilarity from Anglos decreased. Results were interpreted to suggest that Asian enclaves were beginning to form in many U.S. metropolitan areas around 1980.*

## INTRODUCTION

The 1970s were a period of tumultuous change in American cities, as conditions likely to affect the spatial distribution of racial and ethnic groups shifted radically over the decade. Levels and patterns of black, Hispanic, and Asian residential segregation were particularly affected by changes in five areas of national life: federal law, public attitudes, social class, immigration, and the economy.

The year 1968 saw the culmination of a decades-long struggle for black civil rights that progressively dismantled the legal supports for segregation (Farley 1984, pp. 2-5). In the famous *Brown v. Topeka* decision of 1954, the U.S. Supreme Court overturned earlier decisions supporting racial discrimination in public schools. In 1964, the Civil Rights Act banned discrimination in public accommodations, supported the integration of schools, outlawed discrimination involving federal funds, and forbade discrimination in employment. The Voting Rights Act of 1965 attacked the systematic political disenfranchisement of black voters and brought them into the electoral process. Finally, and most important for present purposes, the Civil rights Act of 1968 banned racial discrimination in the sale or rental of housing. Although the 1970 census revealed

little change since 1960 in the very high levels of black segregation (Sorensen et al. 1975; Van Valey et al. 1977), observers at the time noted that fair housing laws had had little time to operate, and looked forward to 1980, when they hoped racial segregation would be reduced.

Over the course of the 1960s and 1970s, there was a sharp improvement in racial attitudes among whites. The percentage of whites opposed to residential integration steadily fell (Greeley and Sheatsley 1974; Taylor et al. 1978), until by 1972 85 percent agreed that it would make no difference to them "if a Negro with just as much income and education" moved onto their block (Pettigrew 1973; 1979). By the mid-1960's, opposition to integration in public and informal settings had almost disappeared (Sheatsley 1966; Greeley and Sheatsley 1974), and over the course of the 1970s, blacks and whites mixed increasingly on the job, in the media, in sports, and in public life generally.

As a result of more tolerant racial attitudes and anti-discrimination legislation, economic opportunities for blacks also increased during the 1970s, leading to the development of a large and increasingly affluent black middle class (Freeman 1976; Wilson 1978; Farley 1984). By 1980 more blacks than ever had access to the levels of income and economic resources that have permitted other groups to achieve spatial assimilation in American society. Some observers suggest that the black middle class largely abandoned poor areas to move into in middle-class neighborhoods, exacerbating the spatial isolation of low-income minority members (Auletta 1982; Wilson 1987).

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Immigration to the United States from Latin America and Asia increased over the past decade. Sweeping changes in U.S. immigration law took effect in 1968 and eliminated the discriminatory national origins quotas (Keely, 1979), and undocumented migration rose markedly. During the 1970s, some 4.5 million legal immigrants and at least 2 million illegal immigrants entered the country (Massey 1981a; Passel and Woodrow 1984; Passel 1986), mostly from Asia and Latin America. They settled primarily in large urban areas such as Los Angeles, New York, Chicago, and Miami, rapidly augmenting Hispanic and Asian populations.

The rapid growth of ethnic and racial minorities through immigration is relevant to residential segregation in two ways. First, it stimulates negative attitudes of natives towards immigrant groups such as Hispanics and Asians (Harwood 1986). The reaction is particularly strong towards undocumented migrants and especially pronounced among native blacks (Harwood 1986; Muller and Espenshade 1985). Second, migration chains tend to concentrate immigrant groups in specific neighborhoods (Massey 1986). New arrivals enter areas where they have friends or relatives. After becoming established, they find permanent homes in the same area, leading to consolidation and further ethnic-enclave growth.

Finally, the 1970s were a decade of urban economic and demographic upheaval. The postwar movement of people and jobs out of central cities into suburban areas continued, although at a slower pace, and the rural-urban shift of population slowed and then reversed itself (Fuguitt 1985). Job creation and population growth during the 1970s were more rapid in nonmetropolitan areas than in the largest urban areas. Some observers argue that these forces increased the spatial isolation of minorities, segregating them within depressed inner-city neighborhoods (Kain 1968; 1974; Kain and Quigley 1975; Straszheim 1980).

It is difficult to say, *a priori*, how these five sets of changes affected racial and ethnic segregation patterns in American cities. Lessening prejudice against blacks and other minorities, the ongoing impact of civil rights legislation, and the rise of the black middle class no doubt acted to reduce segregation. But rapid immigration and metropolitan decentralization probably increased it. In this paper, we measure recent trends in racial and ethnic segregation using 1970 and 1980 census data on blacks, Hispanics, and Asians in 60 large U.S. metropolitan areas, and then analyze interurban variation in the degree of segregation to explain the patterns we observe.

## PATTERNS OF SEGREGATION CIRCA 1970

Studies of blacks in 1970 generally found a high degree of spatial segregation between the races, with indices ranging from about .600 to .900 (using the index of dissimilarity). Segregation was particularly strong in large metropolitan areas with high black concentrations. The average level of black-white segregation in 237 SMSAs in 1970 was .695 (Van Valey et al. 1977), but in the 29 largest urbanized areas it averaged .831 (Massey 1979a). Black segregation did not decline with rising socioeconomic status (Farley 1977; Simkus 1978; Massey 1979b), and average black socioeconomic status was not highly related to interurban variation in black segregation (Massey 1979a; 1981b). However, segregation was related to black-white occupational differentiation in southern cities (Roof et al. 1976). Lieberman and Carter (1982a) estimate that 85 percent of black segregation in 1970 was attributable to involuntary causes.

The pace of black suburbanization increased during the late 1960s and early 1970s (Farley 1970; Rose 1976; Guest 1978; Logan and Schneider 1984), with relatively young and well-educated blacks moving out of central cities into white suburban neighborhoods (Clay 1979; Lake 1981; Spain and Long 1981). But the relative number of suburban blacks in 1970 was quite small, and blacks in suburbs were still quite segregated (Massey 1979a; Logan and Stearns 1981; Schneider and Logan 1982; Logan and Schneider 1984). Nonetheless, several researchers have argued that black suburbanization will eventually lower levels of black residential segregation (Frey 1985; Clark 1986).

Levels and patterns of Hispanic segregation in 1970 differed markedly from those of blacks. Hispanic-Anglo segregation was quite modest in 1970, with segregation scores ranging from .307 to .646 in the 29 urbanized areas studied by Massey (1979a), yielding an average of .444 (again using the index of dissimilarity). In a sample of 35 southwestern central cities, Lopez (1981) found an average score of .545. Segregation declined sharply with rising socioeconomic status and generations spent in the United States (Massey 1979b; Massey 1981c), and was markedly lower in suburbs than in central cities (Massey 1979a). Only the Puerto Rican population of New York contradicted these findings; their segregation was quite high and did not decline with rising socioeconomic status or suburbanization (Massey 1979a; 1979b; 1981b; Jackson 1981). Massey and Bitterman (1985) attributed this pattern to the relatively large number of Puerto Ricans with black ancestry.

Several studies also examined the segregation of blacks and Hispanics using indices of spatial isolation and interaction, which measure the probability of residential contact within and between groups using the  $P^*$  measure popularized by Lieberson (1980; 1981). In urban areas where they are substantial minorities, the two groups display quite different patterns of spatial interaction. Blacks typically experience a low probability of contact with whites and a relatively high degree of spatial isolation, while Hispanics display relatively high probabilities of contact with Anglos and modest spatial isolation (Lieberson and Carter 1981b; Massey and Blakeslee 1983; Massey and Mullan 1984), again with the exception of Puerto Ricans in New York (Massey and Bitterman 1985).

There was no systematic study of Asian segregation done for 1970, but preliminary work from the 1980 census suggests that patterns of Asian segregation closely parallel those of Hispanics, with low-to-moderate levels of segregation from whites that decline with rising socioeconomic status and increasing acculturation (Farley and Langberg 1986; Langberg 1986). These early studies covered only 35 metropolitan areas, however, and were limited in the amount of socioeconomic background information they could consider. To date, no study has systematically compared patterns of black, Hispanic, and Asian segregation across a large sample of urban areas.

#### SOURCES OF DATA

The data used in this study were taken from the 1970 Fourth Count Summary Tapes and the 1980 Summary Tape File 4 (STF4) from the U.S. Bureau of the Census (1970b, 1980). They provide counts of whites, blacks, Hispanics, and Asians in census tracts of Standard Metropolitan Statistical Areas (SMSAs). We selected tracts in the 50 largest SMSAs, plus tracts in 10 other metropolitan areas (mainly in the southwest) that contained relatively large numbers of Hispanics. The 1970 and 1980 tract files were coded to a common grid using a comparability tape from the Census Bureau. Minor boundary shifts that involved population changes of fewer than 100 persons (under 2 percent of an average tract's population) were ignored. When a significant boundary change occurred, tracts in 1970, 1980, or both years were combined until a comparable areal unit was constructed, trying to keep that unit as small as possible. Since areas outside of SMSAs were not generally tracted in 1970, metropolitan areas necessarily follow 1970 definitions. Tracts where more than 40 percent of the 1980 population was American Indian, in the military, or in institutions were deleted from the file to eliminate tracts domi-

nated by Indian reservations, military bases, hospitals, prisons, or other formal institutions.

The definition of mutually exclusive and intercensally comparable groups of whites, blacks, Hispanics, and Asians was problematic. In 1980, we defined the Hispanic population using the Spanish origin item, a 100 percent item asked of all respondents on the short form of the census (U.S. Bureau of the Census 1982). A similar item was asked in 1970, but was included only on the 5 percent sample form, and was worded somewhat differently than in 1980. Because of problems uncovered using this Spanish origin item, we chose to employ the "Spanish American" definition in 1970, which was constructed from 15 percent sample items (U.S. Bureau of the Census 1970a). The Spanish American population includes all persons of Spanish language and, in the five southwestern states, other persons of Spanish surname. People of Spanish language are those who report Spanish as their mother tongue, or who lived in a family where the head reported such a mother tongue.

A systematic comparison of Hispanic segregation indices computed for 1970 using both definitions indicated that in 55 of 60 comparisons, the Spanish origin definition produced a higher level of measured segregation than the Spanish American definition (.520 compared to .444 using index of dissimilarity). This apparent overestimate occurred for two reasons. First, the 5 percent sample missed many Hispanics residing in census tracts where they were few in number. In 39 of the SMSAs under study, the Spanish Origin item yielded significantly fewer "Hispanics" than the Spanish American definition. By missing these relatively integrated Hispanics, segregation scores were biased upwards.

In the remaining 21 SMSAs, the Spanish Origin item produced larger counts of "Hispanics." All of these SMSAs were in the south or central regions and most contained few Hispanics. The Census Bureau suggests that a large number of non-Hispanic people in these regions sought to report themselves as "Americans," and mistakenly gave their origin as "South or Central American" (Siegel and Passel 1979). This error was especially common among blacks. The correlation, across census tracts, between counts of Hispanics identified using the Spanish origin item and those identified using the Spanish American item was only .374 among blacks in southern and central states, but .821 among whites. Since blacks are highly segregated from whites, this misclassification substantially inflates Hispanic segregation scores. The wording of the Spanish Origin item was subsequently changed to eliminate this category,

so this bias does not affect 1980 Spanish origin data.

Given these problems with the 1970 Spanish origin item, we decided to employ the Spanish American definition, which was also the definition employed by Massey in his earlier work on Hispanics using the 1970 census. The term "Hispanic" thus refers to Spanish Americans in 1970 and persons of Spanish origin in 1980. Although comparability is not perfect, it produces reasonable results and is preferable to using the Spanish origin definition in both census years.

Defining Asians was also problematic. Tabulations of Asians by census tract were not prepared for 1970. However, an estimate of the Asian population can be constructed by subtracting blacks and whites from the total population. The residual includes Asians and Pacific Islanders, American Indians, and persons of other race. Because we have eliminated tracts where more than 40 percent of the population was American Indian in 1980, members of the latter group probably do not affect the population of "Asians" to a marked degree. Although Asians, Pacific Islanders, and persons of other race can be identified separately at the tract level in 1980, in the interests of comparability we added the latter two groups back in to match the 1970 residual definition. A comparison of 1980 segregation indices computed with and without this adjustment revealed that it made little difference. Although indices computed using the 1970 definition understated true segregation indices in 58 of 60 cases, the difference averaged only one percentage point and ranged from zero to six points.

A final problem emerges because Hispanics are an ethnic group, while whites, blacks, and Asians are racial groups, and Hispanics can be white, black, or Asian. Fortunately, Hispanics were cross-classified by race in 1970 as well as 1980. White Hispanics, black Hispanics, and Asian Hispanics were therefore subtracted from the respective white, black, and Asian populations; throughout this article the term "Anglo" refers to non-Hispanic whites and the terms "black" and "Asian" indicate non-Hispanic blacks and Asians. We use the term "Asian" to refer to a group that includes Asians, Pacific Islanders, and a very small number of persons of other race.

Obviously, the terms "Hispanic" and "Asian" mask considerable underlying diversity in national origins and characteristics (Massey 1984; Wong 1986; Bean and Tienda 1987). Hispanics encompass relatively well educated, high-income Cubans as well as poorly educated, low-income Puerto Ricans, and Asians run the gamut from Chinese electrical engineers to poor Vietnamese refugees. Both populations display

considerable generational diversity. It is clear that "Hispanics" and "Asians" do not really exist as coherent minority groups except in a weak sense; the census categories are convenient labels that have been externally imposed.

These groups are, nonetheless, those for which the Census Bureau supplies the richest and most detailed information at the census-tract level. Patterns of segregation for individual Asian and Hispanic groups will be studied in future reports. Our strategy here is to use data on Hispanics and Asians to study patterns of ethnic and racial segregation, recognizing the inherent limitations in doing so and trying to adjust interpretations accordingly. In the case of Hispanics, our task is aided somewhat by the fact that the constituent groups have different regional concentrations—Mexicans in the southwest, Puerto Ricans in the northeast, and Cubans in Florida—so that in studying Los Angeles, New York, and Miami we are in some sense isolating Mexicans, Puerto Ricans, and Cubans. In regression models that we later estimate to explain patterns of segregation, the national origin composition of Hispanics and Asians is explicitly controlled.

## MEASURES OF SEGREGATION

There is a voluminous and controversial literature on the measurement of residential segregation (for recent reviews see James and Taeuber 1985, Stearns and Logan 1986, and White 1986). In preparing this study, we undertook a systematic empirical evaluation of all indices of segregation identified from an exhaustive survey of the methodological literature, some 19 in all (Massey and Denton unpublished). Each measure was computed to assess the degree of segregation between blacks, Hispanics, Asians, and Anglos in 1980, and the results were intercorrelated and factor analyzed. Each index was found to load very strongly on one of five underlying factors: evenness, exposure, centralization, concentration, or clustering. This article focuses on the first two of these dimensions, and the remaining dimensions will be covered in other reports.

Evenness is the differential distribution of minority and majority members across census tracts within an urban area. A minority group is said to be segregated if it is unevenly distributed over tracts. Evenness is maximized and segregation minimized when all tracts have the same relative number of minority members as the whole urban area. Residential evenness has typically been measured by the well-known index of dissimilarity:

$$D_{xy} = .5 \sum | (x_i/X) - (y_i/Y) |$$

where  $x_i$  and  $y_i$  are the numbers of  $X$  and  $Y$  members in tract  $i$ , and  $X$  and  $Y$  are their city-wide totals. Several other measures of evenness have been devised and are advocated by different researchers (James and Taeuber 1985; White 1986), but in our empirical analysis, all evenness measures were highly intercorrelated with one another (with values of  $r^2$  ranging from .92 to .98) and all loaded on the same underlying axis in our factor analysis (Massey and Denton unpublished). Given the apparent empirical equivalence of the various measures, we selected the index of dissimilarity to maintain comparability with work done in 1970 and before, nearly all of which used  $D$ .

The second dimension is exposure, which refers to the degree of potential contact between minority and majority members within census tracts of urban areas. Exposure indices measure the extent to which minority and majority members must physically confront one another by virtue of sharing a common tract of residence. The degree of minority exposure may be conceptualized as the likelihood that minority and majority members share a common neighborhood. The most widely used measure of exposure is the  $P^*$  index recently popularized by Lieberman (1980; 1981). The probability of residential contact between groups  $X$  and  $Y$  is estimated as

$${}_xP^*y = \sum (x_i/X) * (y_i/t_i),$$

where  $t_i$  is the total population of tract  $i$  and the other variables are denoted as before.  $P^*$  is an asymmetric measure of segregation because its value depends partially on the relative number of  $X$  and  $Y$  members in the urban area, and, except when  $X=Y$ ,  ${}_xP^*y$  will not equal  ${}_yP^*x$ . Thus, for any two groups there are four possible exposure indices:  ${}_xP^*x$ ,  ${}_xP^*y$ ,  ${}_yP^*x$ , and  ${}_yP^*y$ .

In general,  ${}_xP^*x$  and  ${}_yP^*y$  are called isolation indices and  ${}_xP^*y$  and  ${}_yP^*x$  are called interaction indices. The value of  ${}_xP^*x$  can be standardized to control for the effect of population composition, producing a symmetrical measure of segregation that is equivalent to the correlation ratio:

$$\eta^2 = [{}_xP^*x - (X/T)]/[1 - (X/T)].$$

Stearns and Logan (1986) argue that  $\eta^2$  measures an independent dimension of residential segregation, but our factor analysis showed it to load on the same dimension as  ${}_xP^*x$  and  ${}_xP^*y$ , and to be highly correlated with them ( $r^2 = .76$ ). We chose  $P^*$  as our measure of intergroup exposure because of its straightforward probabilistic interpretation and its use by several researchers in 1970.

## TRENDS IN INTERGROUP EXPOSURE

One cannot interpret trends in intergroup exposure without considering changes in population size and ethnic composition in the 60 SMSAs under study. Unfortunately, limitations of space prevent us from tabulating this information here, so we refer to an unpublished table giving the number and proportion of minority and majority members in each metropolitan area (available on request). These data reveal a widespread decline in Anglo population relative to blacks, Hispanics, and Asians. In the 60 SMSAs, Anglos increased by 233,000 persons, compared to respective increases for blacks, Hispanics, and Asians of 2.5 million, 3.0 million and 1.8 million. As a result, Anglos' share of the metropolitan population fell by an average of 5 percentage points, while the other groups' share increased from 1.5 to 2.4 points. The percentage of Anglos fell in 56 of the 60 SMSAs, and Anglos sustained absolute losses in 24 cases. By way of contrast, the percentage of blacks fell in only 8 cases, and the percentage of Hispanics in 10, and no SMSA reported absolute declines for these groups. All SMSAs showed an increase in the percentage Asian, but most began the decade with very small Asian populations.

Over the decade of the 1970s, minorities became increasingly preponderant in most SMSAs. By 1980 blacks exceeded 20 percent of the metropolitan population in 12 of the areas under study, including Chicago and New York. They comprised 40 percent of metropolitan Memphis and 32 percent of metropolitan New Orleans. Hispanics similarly exceeded 20 percent of the population in 10 SMSAs, including Los Angeles and Miami, which experienced particularly strong shifts in composition. In both areas the percentage Anglo fell by 15 percentage points, while the percentage of Hispanics grew by 9 and 12 points, respectively. Hispanics were majorities or near majorities in Corpus Christi, El Paso, and San Antonio. Asians represented a much smaller share of the population in all cities. They constituted the largest proportion in San Francisco, where they were 11 percent of the population, followed by 9 percent in San Jose. Asians exceeded 5 percent of the metropolitan population in seven cases, all on the west coast, and, with the exception of Seattle, all in California.

Table 1 presents 1970 and 1980 indicators of residential exposure ( $P^*$ ) for minority and majority groups in the 60 SMSAs. To conserve space, 1970–1980 changes are not shown. Since the information in this table is substantial, we focus primarily on the averages at the bottom of the table, and on five "key metropolitan areas" that contain significant numbers of all three

Table 1. Probabilities of Residential Contact Between Blacks, Hispanics, Asians, and Anglos in 60 Metropolitan Areas, 1970-1980

Metropolitan Area and Group	Group's Probability of Contact with:							
	Anglos		Blacks		Hispanics		Asians	
	1970	1980	1970	1980	1970	1980	1970	1980
<b>KEY METROPOLITAN AREAS</b>								
<i>Chicago</i>								
Anglo	.926	.881	.027	.036	.040	.059	.008	.026
Black	.118	.125	.855	.828	.023	.038	.005	.009
Hispanic	.649	.499	.085	.093	.251	.380	.021	.031
Asian	.721	.736	.099	.075	.109	.105	.076	.087
<i>Los Angeles-Long Beach</i>								
Anglo	.812	.720	.023	.038	.141	.180	.027	.065
Black	.153	.165	.703	.604	.110	.188	.037	.046
Hispanic	.523	.347	.063	.084	.378	.501	.046	.074
Asian	.534	.489	.112	.080	.242	.286	.123	.152
<i>Miami</i>								
Anglo	.766	.649	.042	.074	.189	.263	.004	.014
Black	.174	.206	.752	.642	.073	.143	.002	.011
Hispanic	.487	.341	.045	.067	.465	.583	.005	.011
Asian	.618	.526	.088	.146	.289	.311	.008	.018
<i>New York</i>								
Anglo	.862	.820	.051	.056	.076	.092	.012	.033
Black	.210	.164	.588	.627	.193	.189	.013	.021
Hispanic	.389	.330	.236	.232	.361	.400	.022	.040
Asian	.558	.556	.136	.119	.197	.184	.116	.143
<i>San Francisco-Oakland</i>								
Anglo	.805	.763	.041	.053	.109	.095	.052	.095
Black	.292	.299	.560	.511	.098	.104	.059	.092
Hispanic	.671	.582	.085	.113	.192	.193	.066	.120
Asian	.587	.564	.095	.097	.121	.116	.210	.232
<b>OTHER METROPOLITAN AREAS</b>								
<i>Albany-Schenectady-Troy</i>								
Anglo	.966	.955	.022	.028	.007	.008	.004	.009
Black	.704	.690	.282	.279	.008	.015	.005	.016
Hispanic	.948	.909	.036	.066	.012	.014	.004	.011
Asian	.944	.908	.041	.066	.008	.010	.007	.017
<i>Albuquerque</i>								
Anglo	.679	.662	.012	.017	.297	.292	.015	.032
Black	.436	.516	.097	.051	.454	.388	.015	.050
Hispanic	.425	.448	.019	.019	.544	.506	.016	.030
Asian	.543	.578	.016	.029	.420	.348	.026	.048
<i>Anaheim-Santa Ana-Garden Grove</i>								
Anglo	.881	.821	.004	.010	.101	.115	.017	.056
Black	.509	.651	.174	.038	.305	.241	.026	.074
Hispanic	.773	.610	.017	.020	.194	.310	.021	.064
Asian	.839	.750	.009	.015	.131	.161	.026	.077
<i>Atlanta</i>								
Anglo	.925	.886	.062	.091	.011	.013	.002	.011
Black	.213	.237	.780	.748	.005	.010	.002	.005
Hispanic	.860	.736	.115	.228	.022	.022	.003	.014
Asian	.819	.821	.161	.144	.013	.018	.007	.018
<i>Austin</i>								
Anglo	.853	.797	.059	.059	.104	.128	.004	.018
Black	.269	.397	.524	.368	.206	.226	.002	.011
Hispanic	.514	.524	.148	.137	.336	.326	.002	.019
Asian	.827	.724	.082	.069	.101	.186	.011	.023
<i>Bakersfield</i>								
Anglo	.847	.790	.017	.026	.124	.151	.014	.036
Black	.248	.365	.438	.346	.250	.262	.021	.032
Hispanic	.557	.481	.077	.059	.349	.421	.031	.046
Asian	.597	.646	.055	.040	.303	.257	.062	.066

Table 1. (Continued)

Metropolitan Area and Group	Group's Probability of Contact with:							
	Anglos		Blacks		Hispanics		Asians	
	1970	1980	1970	1980	1970	1980	1970	1980
<i>Baltimore</i>								
Anglo	.919	.887	.067	.090	.009	.009	.005	.015
Black	.216	.259	.772	.723	.008	.009	.004	.009
Hispanic	.753	.705	.224	.263	.017	.015	.006	.017
Asian	.789	.785	.186	.178	.011	.011	.015	.026
<i>Birmingham</i>								
Anglo	.768	.787	.227	.201	.004	.007	.001	.005
Black	.544	.486	.451	.502	.004	.007	.001	.004
Hispanic	.669	.691	.319	.295	.009	.009	.003	.004
Asian	.674	.728	.309	.257	.007	.007	.007	.007
<i>Boston</i>								
Anglo	.966	.946	.017	.022	.011	.018	.007	.015
Black	.376	.345	.567	.551	.045	.080	.013	.025
Hispanic	.787	.653	.145	.185	.052	.129	.017	.033
Asian	.822	.768	.071	.080	.028	.047	.080	.105
<i>Buffalo</i>								
Anglo	.961	.946	.024	.035	.010	.011	.005	.009
Black	.267	.335	.712	.635	.016	.022	.006	.009
Hispanic	.820	.751	.118	.157	.050	.077	.012	.016
Asian	.875	.867	.082	.087	.025	.022	.019	.024
<i>Cincinnati</i>								
Anglo	.943	.926	.049	.062	.005	.005	.003	.007
Black	.401	.441	.591	.543	.004	.009	.004	.007
Hispanic	.890	.799	.096	.183	.010	.010	.004	.008
Asian	.823	.854	.162	.127	.006	.007	.008	.013
<i>Cleveland</i>								
Anglo	.952	.936	.033	.041	.010	.013	.005	.010
Black	.170	.180	.819	.804	.007	.010	.004	.006
Hispanic	.815	.771	.114	.133	.065	.082	.008	.015
Asian	.837	.836	.131	.122	.018	.022	.015	.021
<i>Columbus</i>								
Anglo	.944	.917	.046	.065	.006	.008	.004	.011
Black	.355	.407	.635	.575	.007	.009	.003	.010
Hispanic	.850	.822	.131	.152	.014	.013	.005	.014
Asian	.891	.832	.090	.130	.008	.010	.011	.029
<i>Corpus Christi</i>								
Anglo	.704	.643	.009	.015	.286	.333	.003	.010
Black	.116	.186	.363	.267	.517	.544	.009	.005
Hispanic	.321	.317	.043	.042	.635	.636	.002	.006
Asian	.545	.591	.109	.021	.344	.377	.009	.015
<i>Dallas-Fort Worth</i>								
Anglo	.908	.866	.033	.050	.055	.068	.006	.016
Black	.189	.272	.760	.646	.049	.074	.003	.009
Hispanic	.700	.619	.107	.124	.186	.240	.009	.018
Asian	.818	.796	.069	.078	.098	.101	.017	.026
<i>Dayton</i>								
Anglo	.960	.937	.031	.049	.005	.006	.003	.008
Black	.258	.339	.733	.650	.005	.007	.004	.005
Hispanic	.878	.837	.106	.145	.012	.010	.005	.008
Asian	.867	.904	.118	.077	.007	.007	.008	.012
<i>Denver-Boulder</i>								
Anglo	.890	.867	.011	.026	.090	.086	.011	.022
Black	.235	.455	.596	.411	.153	.110	.020	.026
Hispanic	.661	.649	.052	.048	.274	.275	.018	.031
Asian	.753	.773	.063	.054	.166	.143	.022	.033
<i>Detroit</i>								
Anglo	.936	.919	.047	.054	.013	.015	.004	.013
Black	.222	.204	.759	.773	.014	.015	.006	.009
Hispanic	.775	.736	.173	.186	.046	.065	.007	.015
Asian	.753	.806	.217	.153	.020	.019	.011	.023



Table 1. (Continued)

Metropolitan Area and Group	Group's Probability of Contact with:							
	Anglos		Blacks		Hispanics		Asians	
	1970	1980	1970	1980	1970	1980	1970	1980
<i>El Paso</i>								
Anglo	.550	.480	.015	.033	.427	.470	.009	.020
Black	.337	.405	.053	.050	.604	.525	.007	.022
Hispanic	.266	.229	.017	.021	.715	.741	.006	.011
Asian	.488	.435	.016	.040	.485	.506	.016	.023
<i>Fort Lauderdale</i>								
Anglo	.954	.919	.016	.034	.026	.040	.004	.008
Black	.113	.262	.873	.702	.013	.031	.002	.005
Hispanic	.893	.852	.064	.087	.039	.053	.004	.009
Asian	.886	.866	.072	.075	.030	.047	.011	.013
<i>Fresno</i>								
Anglo	.754	.713	.014	.025	.206	.221	.029	.045
Black	.210	.313	.522	.377	.248	.278	.026	.039
Hispanic	.551	.469	.046	.046	.376	.446	.033	.045
Asian	.640	.618	.040	.042	.273	.292	.057	.053
<i>Gary-Hammond-E. Chicago</i>								
Anglo	.927	.901	.028	.038	.042	.054	.003	.007
Black	.121	.142	.804	.773	.073	.081	.002	.004
Hispanic	.552	.538	.223	.220	.223	.237	.005	.006
Asian	.804	.815	.111	.107	.079	.065	.006	.013
<i>Greensboro-Winston Salem</i>								
Anglo	.896	.867	.097	.121	.004	.006	.003	.007
Black	.432	.487	.561	.501	.003	.007	.004	.006
Hispanic	.835	.765	.151	.217	.010	.011	.004	.007
Asian	.772	.810	.207	.171	.005	.007	.016	.013
<i>Houston</i>								
Anglo	.836	.783	.070	.078	.089	.116	.006	.025
Black	.254	.283	.664	.593	.078	.109	.005	.016
Hispanic	.585	.517	.141	.134	.269	.328	.011	.022
Asian	.659	.699	.149	.120	.184	.137	.015	.045
<i>Indianapolis</i>								
Anglo	.941	.929	.049	.057	.007	.007	.003	.007
Black	.346	.361	.645	.623	.007	.009	.003	.007
Hispanic	.874	.824	.109	.156	.014	.012	.003	.008
Asian	.861	.852	.125	.127	.008	.009	.005	.013
<i>Jersey City</i>								
Anglo	.826	.717	.051	.052	.118	.201	.007	.031
Black	.394	.260	.528	.604	.073	.111	.008	.027
Hispanic	.602	.456	.048	.050	.345	.465	.010	.032
Asian	.696	.566	.098	.100	.190	.260	.021	.078
<i>Kansas City</i>								
Anglo	.945	.921	.033	.044	.019	.023	.004	.013
Black	.236	.282	.742	.690	.017	.020	.004	.009
Hispanic	.798	.773	.103	.106	.092	.104	.007	.019
Asian	.823	.840	.113	.094	.035	.037	.009	.030
<i>Louisville</i>								
Anglo	.950	.929	.043	.058	.005	.006	.002	.007
Black	.308	.357	.687	.633	.003	.005	.002	.005
Hispanic	.912	.851	.076	.132	.009	.009	.003	.009
Asian	.896	.871	.092	.106	.006	.008	.007	.015
<i>Memphis</i>								
Anglo	.858	.815	.132	.169	.006	.008	.003	.009
Black	.214	.228	.780	.759	.004	.009	.002	.004
Hispanic	.705	.524	.281	.456	.011	.013	.003	.007
Asian	.695	.728	.294	.248	.005	.010	.006	.015
<i>Milwaukee</i>								
Anglo	.961	.934	.020	.034	.014	.021	.005	.012
Black	.239	.269	.739	.695	.016	.026	.007	.011
Hispanic	.817	.706	.076	.112	.098	.162	.010	.022
Asian	.863	.838	.091	.094	.028	.045	.018	.025

Table 1. (Continued)

Metropolitan Area and Group	Group's Probability of Contact with:							
	Anglos		Blacks		Hispanics		Asians	
	1970	1980	1970	1980	1970	1980	1970	1980
<i>Minneapolis-St. Paul</i>								
Anglo	.973	.953	.009	.017	.069	.011	.009	.020
Black	.552	.622	.399	.307	.022	.021	.028	.051
Hispanic	.898	.870	.039	.047	.049	.048	.014	.039
Asian	.908	.861	.048	.059	.014	.020	.030	.062
<i>Nashville-Davidson</i>								
Anglo	.927	.901	.064	.086	.006	.006	.003	.007
Black	.296	.375	.697	.611	.004	.008	.002	.006
Hispanic	.869	.751	.114	.229	.013	.012	.004	.009
Asian	.818	.814	.161	.159	.010	.008	.012	.019
<i>Nassau-Suffolk</i>								
Anglo	.941	.924	.026	.030	.029	.035	.004	.012
Black	.538	.445	.412	.469	.045	.073	.006	.014
Hispanic	.871	.781	.066	.111	.060	.096	.005	.013
Asian	.895	.867	.062	.069	.034	.043	.010	.022
<i>New Orleans</i>								
Anglo	.829	.797	.120	.141	.047	.047	.003	.015
Black	.257	.274	.713	.688	.028	.027	.002	.012
Hispanic	.720	.708	.203	.212	.074	.063	.003	.017
Asian	.723	.613	.220	.251	.049	.046	.008	.093
<i>Newark</i>								
Anglo	.906	.872	.056	.065	.033	.046	.005	.018
Black	.261	.218	.670	.692	.064	.078	.007	.012
Hispanic	.581	.481	.245	.241	.167	.263	.010	.016
Asian	.686	.757	.217	.149	.085	.066	.015	.029
<i>Norfolk-Virginia Beach-Portsmouth</i>								
Anglo	.879	.809	.102	.155	.011	.015	.009	.024
Black	.254	.351	.735	.628	.007	.010	.004	.012
Hispanic	.782	.736	.182	.217	.022	.020	.016	.030
Asian	.775	.767	.137	.173	.021	.019	.069	.047
<i>Oklahoma City</i>								
Anglo	.941	.893	.019	.044	.017	.023	.024	.041
Black	.203	.386	.772	.569	.014	.021	.010	.026
Hispanic	.867	.810	.069	.087	.038	.056	.027	.049
Asian	.901	.854	.038	.062	.020	.029	.042	.057
<i>Paterson-Clifton-Passaic</i>								
Anglo	.899	.874	.049	.043	.048	.071	.004	.012
Black	.377	.240	.485	.489	.135	.260	.006	.011
Hispanic	.579	.368	.209	.241	.207	.375	.010	.016
Asian	.706	.690	.140	.110	.147	.170	.013	.031
<i>Philadelphia</i>								
Anglo	.921	.912	.062	.061	.013	.016	.004	.012
Black	.287	.257	.682	.696	.025	.035	.006	.013
Hispanic	.634	.500	.254	.267	.106	.216	.009	.017
Asian	.720	.743	.225	.185	.032	.033	.024	.040
<i>Phoenix</i>								
Anglo	.868	.866	.012	.018	.108	.096	.014	.021
Black	.311	.484	.385	.225	.291	.265	.020	.030
Hispanic	.602	.591	.064	.062	.321	.321	.026	.033
Asian	.703	.738	.039	.039	.237	.186	.043	.048
<i>Pittsburgh</i>								
Anglo	.958	.952	.034	.037	.005	.005	.003	.006
Black	.454	.446	.535	.541	.006	.008	.004	.005
Hispanic	.888	.861	.095	.119	.013	.013	.004	.007
Asian	.890	.908	.094	.070	.006	.007	.011	.016
<i>Portland</i>								
Anglo	.960	.934	.012	.017	.013	.019	.014	.030
Black	.538	.606	.426	.316	.019	.026	.017	.054
Hispanic	.931	.904	.030	.036	.023	.028	.016	.033
Asian	.931	.889	.027	.045	.016	.020	.027	.047

Table 1. (Continued)

Metropolitan Area and Group	Group's Probability of Contact with:							
	Anglos		Blacks		Hispanics		Asians	
	1970	1980	1970	1980	1970	1980	1970	1980
<i>Providence-Warwick-Pawtucket</i>								
Anglo	.973	.956	.016	.017	.006	.016	.004	.012
Black	.717	.601	.254	.253	.012	.086	.017	.063
Hispanic	.932	.769	.041	.110	.019	.085	.008	.039
Asian	.895	.781	.075	.113	.011	.057	.020	.047
<i>Riverside-San Bernardino-Ontario</i>								
Anglo	.829	.782	.026	.037	.135	.152	.013	.031
Black	.515	.586	.254	.160	.222	.226	.016	.032
Hispanic	.636	.601	.052	.057	.302	.316	.017	.031
Asian	.739	.733	.045	.043	.200	.182	.025	.041
<i>Rochester</i>								
Anglo	.952	.929	.032	.047	.011	.014	.005	.010
Black	.460	.487	.495	.442	.040	.061	.005	.010
Hispanic	.725	.614	.188	.254	.082	.120	.006	.013
Asian	.905	.875	.061	.08	.015	.025	.017	.019
<i>Sacramento</i>								
Anglo	.848	.817	.030	.043	.091	.088	.035	.055
Black	.557	.577	.260	.209	.136	.138	.055	.083
Hispanic	.736	.691	.059	.080	.163	.165	.048	.069
Asian	.707	.699	.060	.078	.122	.111	.118	.116
<i>St. Louis</i>								
Anglo	.944	.927	.043	.055	.010	.009	.004	.009
Black	.227	.257	.765	.729	.006	.009	.003	.005
Hispanic	.877	.804	.098	.168	.022	.019	.004	.010
Asian	.848	.865	.132	.109	.011	.011	.009	.016
<i>Salt Lake City-Ogden</i>								
Anglo	.937	.924	.005	.007	.046	.046	.014	.024
Black	.707	.815	.111	.041	.152	.108	.033	.037
Hispanic	.868	.861	.019	.018	.096	.089	.019	.034
Asian	.899	.884	.014	.012	.065	.066	.024	.040
<i>San Antonio</i>								
Anglo	.698	.660	.021	.043	.276	.283	.007	.015
Black	.159	.297	.511	.360	.328	.334	.005	.010
Hispanic	.279	.276	.044	.046	.675	.670	.007	.009
Asian	.470	.573	.042	.055	.482	.354	.012	.020
<i>San Diego</i>								
Anglo	.851	.808	.016	.029	.115	.117	.024	.052
Black	.323	.421	.419	.263	.221	.233	.061	.094
Hispanic	.708	.582	.069	.079	.198	.269	.040	.080
Asian	.689	.631	.088	.076	.187	.194	.059	.111
<i>San Jose</i>								
Anglo	.811	.763	.012	.026	.146	.135	.036	.082
Black	.597	.565	.059	.066	.308	.260	.046	.120
Hispanic	.645	.547	.027	.048	.296	.317	.040	.099
Asian	.746	.655	.019	.043	.189	.194	.053	.116
<i>Seattle-Everett</i>								
Anglo	.946	.914	.013	.021	.016	.019	.025	.048
Black	.446	.544	.427	.294	.020	.030	.108	.138
Hispanic	.894	.854	.033	.052	.031	.026	.043	.072
Asian	.762	.768	.099	.087	.025	.026	.117	.124
<i>Tampa-St. Petersburg</i>								
Anglo	.907	.895	.046	.051	.044	.046	.003	.008
Black	.361	.422	.580	.515	.058	.056	.002	.007
Hispanic	.642	.706	.106	.104	.250	.182	.003	.009
Asian	.871	.840	.067	.089	.056	.058	.005	.013
<i>Tucson</i>								
Anglo	.821	.812	.017	.021	.154	.147	.012	.023
Black	.481	.611	.194	.088	.313	.276	.023	.033
Hispanic	.485	.509	.035	.033	.467	.431	.032	.039
Asian	.520	.628	.034	.031	.420	.305	.067	.050

Table 1. (Continued)

Metropolitan Area and Group	Group's Probability of Contact with:							
	Anglos		Blacks		Hispanics		Asians	
	1970	1980	1970	1980	1970	1980	1970	1980
<i>Washington, DC</i>								
Anglo	.892	.812	.070	.118	.027	.033	.011	.039
Black	.208	.280	.772	.680	.014	.022	.007	.020
Hispanic	.803	.705	.142	.196	.043	.054	.013	.047
Asian	.777	.741	.169	.162	.033	.042	.022	.057
<i>Average</i>								
Anglo	.883	.849	.040	.053	.068	.077	.010	.023
Black	.333	.376	.553	.491	.103	.110	.014	.026
Hispanic	.709	.642	.106	.131	.173	.201	.014	.028
Asian	.760	.749	.104	.099	.108	.107	.032	.047

minority groups: Chicago, Los Angeles, Miami, New York, and San Francisco. Discussion of other metropolitan areas will be general, except when a particular pattern of segregation draws our attention.

Of the many residential contact probabilities shown in Table 1, two are especially revealing: the isolation index, or  $xP^*_x$ , which measures the average probability of group  $X$  members sharing a tract with themselves (i.e., blacks with blacks, Hispanics with Hispanics, or Asians with Asians); and the interaction index, or  $xP^*_y$ , which measures the probability that group  $X$  members have of sharing a tract with  $Y$  members, in this case Anglos (i.e., blacks with Anglos, Hispanics with Anglos, or Asians with Anglos). The increasing prevalence of minorities in nearly all of the SMSAs suggests a decrease in Anglo-interaction probabilities and an increase in isolation indices, other things being equal.

Black trends run opposite this prediction, however, suggesting there has been some improvement in their spatial position over the past decade. The probability of contact with other blacks declined by an average of .062 over the decade, from .553 in 1970 to .491 in 1980, while the average probability of interaction with Anglos increased from .333 to .376. Black isolation decreased in 50 of the 60 SMSAs and the likelihood of Anglo contact increased in 47 cases. Given that the effect of compositional change was in the opposite direction, one might be led to conclude that civil rights legislation and more tolerant white attitudes finally had an effect in decreasing levels of black segregation in U.S. cities.

This initial optimism is dispelled somewhat by a closer look at the data. In spite of declines over the past decade, blacks remain by far the most spatially isolated of the three minority groups. Average black isolation (.491) is 2.5 times that of Hispanics (.201) and 10 times that of Asians (.047). In some SMSAs, such as

Chicago, the level of black isolation (.828) is extremely high and has changed little over the past decade (it was .855 in 1970). The probability of residential contact with Anglos was only .125, which is actually *lower* than the proportion of blacks in Chicago (.199), indicating that a very rigid pattern of racial segregation must have been imposed. Even in El Paso, where Hispanics make up roughly 66 percent of the SMSA, the likelihood of Anglo contact was .299.

The high degree of black spatial isolation is put into better perspective when average contact probabilities are tabulated by basic SMSA characteristics. The first three columns of Table 2 show black-Anglo interaction probabilities classified by region, SMSA size, minority population size, total population growth, and rate of minority immigration, where the latter variable is defined as the intercensal rate of growth in the foreign-born population. (In this and subsequent tables, the "minority" in the rows for "minority population size" and "minority immigration" refers to specific minority groups, rather than all minorities added together: blacks in the black columns, Hispanics in the Hispanic columns, and Asians in the Asian columns.) All variables except region are classified into quartiles of 15 SMSAs each.

This table reveals that increases in the probability of black-Anglo interaction were by no means general across U.S. metropolitan areas. They occurred primarily outside the largest size category in rapidly growing SMSAs of the south and west that contained relatively few blacks. The greatest increases were in places like Anaheim, Austin, Bakersfield, Denver, Ft. Lauderdale, Fresno, Oklahoma City, and Portland. The large, slowly growing metropolitan areas of the northeast and north central states, where the vast majority of urban blacks live, did not show marked changes. Places like Baltimore, Chicago, Cleveland, and St. Louis had very low Anglo contact probabil-

Table 2. Average Minority-Anglo Interaction Probabilities in 60 SMSAs Classified by Selected Metropolitan Characteristics, 1970-1980

Metropolitan Characteristic	Minority Group								
	Blacks			Hispanics			Asians		
	1970	1980	Change	1970	1980	Change	1970	1980	Change
<i>Region</i>									
Northeast	.421	.374	-.047	.730	.623	-.107	.799	.774	-.025
North Central	.270	.302	.032	.806	.748	-.058	.833	.837	.003
South	.256	.322	.066	.673	.618	-.055	.741	.729	-.012
West	.410	.498	.088	.667	.608	-.059	.699	.690	-.009
<i>SMSA Population</i>									
Largest	.267	.268	.001	.699	.608	-.091	.737	.739	.002
Bigger	.352	.424	.072	.742	.676	-.066	.786	.764	-.022
Smaller	.387	.429	.042	.786	.731	-.055	.812	.797	-.014
Smallest	.325	.382	.056	.610	.554	-.056	.705	.696	-.009
<i>Minority Population</i>									
Largest	.219	.231	.013	.572	.488	-.084	.707	.689	-.018
Bigger	.336	.355	.018	.619	.564	-.055	.796	.777	-.019
Smaller	.328	.395	.067	.812	.748	-.064	.757	.756	-.001
Smallest	.449	.522	.073	.834	.770	-.064	.779	.774	-.005
<i>Rate of Population Growth</i>									
Fastest	.372	.472	.100	.619	.580	-.038	.716	.700	-.016
Faster	.314	.383	.069	.699	.644	-.054	.733	.730	-.003
Slower	.345	.356	.012	.781	.696	-.084	.796	.776	-.020
Slowest	.301	.290	-.010	.739	.648	-.091	.795	.790	-.005
<i>Rate of Minority Immigration</i>									
Highest	.285	.362	.078	.747	.656	-.092	.781	.770	-.011
Higher	.395	.434	.039	.780	.715	-.065	.768	.757	-.011
Slower	.329	.368	.038	.740	.678	-.062	.784	.786	.002
Slowest	.321	.338	.017	.570	.522	-.048	.706	.684	-.022
Average	.333	.376	.043	.709	.642	-.067	.760	.749	-.011

Note: Number of SMSAs in different regions are as follows: Northeast (14), North Central (11), South (19), West (16). The remaining variables are classified into equal quartiles of 15 SMSAs each.

ities ranging from .125 to .257, with little change over the decade. In Detroit, Newark, New York, and Philadelphia, the likelihood of contact with Anglos actually *decreased*. The only real exception to this pattern was Washington, DC, where "gentrification" apparently was responsible for a decrease in black spatial isolation (Lee et al. 1985), although it is arguable whether this type of integration represents a stable outcome. In short, there is little evidence that large black ghettos in the north became less isolated spatially from the mainstream of American society during the 1970s. Integration occurred primarily in small and mid-sized cities that contained relatively few blacks.

At first glance, recent trends in Hispanic exposure probabilities also provide cause for pessimism. Increasing spatial isolation and declining contact with Anglos was the most common pattern for Hispanics during the 1970s, holding in about half of the SMSAs listed in Table 1. The average level of Hispanic isolation rose from .173 to .201, and the probability of Anglo interaction fell from .709 to .642. There was considerable variation around this pattern, however, and as Table 2 shows, large declines in the probability of Anglo interaction were by

no means general. They were concentrated particularly in the northeast, in large metropolitan areas with relatively low rates of total population growth, in large Hispanic populations, or in areas with high rates of Hispanic immigration.

In other words, declining Hispanic contact with Anglos appears to have been a consequence of shifting population composition. Hispanic spatial isolation rose markedly when a large and rapidly growing Hispanic population combined with a declining Anglo population to cause a rapid increase in the proportion Hispanic. Chicago, Los Angeles, Anaheim, Miami, Paterson, Newark, and Jersey City all experienced sizeable absolute and relative increases in their Hispanic populations (data not shown). Not surprisingly, each area also recorded a decline in the likelihood of contact with Anglos and an increase in the degree of Hispanic isolation.

Even after these pronounced increases, moreover, Hispanic isolation indices remain considerably below those of blacks. Whereas Hispanic isolation in Chicago increased from .251 to .380, the 1980 figure for blacks was .828. In Los Angeles the Hispanic isolation index of .501 still compares favorably to the black index of .604, even though Hispanics are a much

larger share of the population than blacks (28 percent compared to 12 percent, so that, other things equal, they should be *more* isolated than blacks). Similar contrasts stand out in Miami, Jersey City, Newark, Paterson, and other urban areas with large and growing Hispanic populations.

With respect to exposure probabilities, Asians stand distinctly apart from blacks and Hispanics because they represent a much smaller share of the population in most SMSAs. Even in San Francisco, Asians are only 11 percent of the metropolitan population, and their average over all SMSAs is just under 3 percent. Naturally, with such small relative numbers in all metropolitan areas, Asians experience very low levels of spatial isolation and very high likelihoods of Anglo contact. The average isolation index in 1980 was only .047, and the average probability of Asian interaction with Anglos was .749. The maximum isolation index occurred, of course, in San Francisco, where it stood at .232 in 1980, up from .210 a decade before. The probability of interaction with Anglos was .564. Similarly, in Los Angeles and New York, isolation indices were around .150, and Anglo interaction probabilities were in the range of .500 to .550.

In the largest Asian concentrations in the United States, therefore, the probability of sharing a census tract with another Asian was always less than .250, while the likelihood of sharing a tract with an Anglo was always .500 or greater. Obviously, Asian enclaves exist in some SMSAs, but it seems clear that most Asians do not live in them. Rather, they display a remarkably high level of spatial assimilation and little isolation. Given these facts, and the relative paucity of Asians in most SMSAs, it is not surprising that the classifications of Table 2 reveal few meaningful patterns. The pattern everywhere seems to be one of small increases from a very low level of spatial isolation in 1970, accompanied by modest decreases in the probability of Asian-Anglo interaction.

#### TRENDS IN RESIDENTIAL DISSIMILARITY

Unlike the exposure indices reported above, the index of dissimilarity has no *mathematical* relationship to the minority composition of the population (Duncan and Duncan 1955), but it may have a *behavioral* relationship (Lieberson 1980; Lieberson and Carter 1982a). If one assumes that Anglos desire to minimize contact with minorities, then the relative number of minority members has profound implications for spatial behavior. Suppose that all Anglos are willing to tolerate no more than a .10 probability of residential contact with blacks. If blacks are 10 percent of the urban population, then this

desire can be satisfied without imposing residential dissimilarity. With each tract 10 percent black, all Anglos are satisfied and the index of dissimilarity is 0. If blacks are 40 percent of the population, however, spatial unevenness has to be imposed on blacks. If no tract containing whites is to exceed 10 percent black, then some tracts will have to be all black, leading to high indices of residential dissimilarity.

Other things equal, therefore, a rising minority percentage within an urban area leads to rising levels of dissimilarity if Anglos seek to limit residential contact with minorities. But other things are rarely equal, and a rising minority percentage may also be associated with lower indices of dissimilarity if the shift in composition stems from rapid minority immigration. When existing minority enclaves cannot accommodate new migrants, they must settle in predominantly majority areas. Even if minority entry leads ultimately to Anglo population loss and eventual succession, in the short run a decline in segregation may ensue, as happened for blacks in many northern cities between 1950 and 1960 (Taeuber and Taeuber 1965).

During the 1970s, black migration to northern cities virtually ceased (Wilson 1981), so the decline in black-Anglo dissimilarity shown in Table 3 is especially noteworthy. Most metropolitan areas experienced a clear lowering of black segregation over the decade (in 54 of 60 cases), and on average, black segregation fell by almost .100, from .792 to .694, suggesting substantial progress in the desegregation of U.S. metropolitan areas. Again, however, a closer look at the data dampens optimism about the extent of recent racial integration.

Table 4 shows average dissimilarities classified by selected metropolitan characteristics. Although black segregation declined in all metropolitan categories, the decline was strongest in the south and the west, especially the latter. As with the exposure indices, the largest declines were registered in rapidly growing urban areas with small black populations, places such as Albuquerque, Anaheim, and Austin, rather than in large black concentrations such as Chicago, Detroit, or Cleveland. Large, declining metropolitan areas in the northeast and north central regions, which house the majority of urban blacks, remained very segregated and displayed relatively little change over the decade, especially in the northeast. The 30 largest black SMSAs averaged a decline of only -.050, compared to an average change of -.116 in the next smallest quartile and -.173 in the smallest. In Cleveland, Los Angeles, New York, Detroit, Newark, and St. Louis, the index of dissimilarity remained well above .800 with changes in the range of -.010 to -.040. In New York, Newark, Jersey City, Paterson, and

Table 3. Residential Dissimilarity of Blacks, Hispanics, and Asians from Anglos in 60 U.S. Metropolitan Areas, 1970-1980

Metropolitan Area	Dissimilarity Between Anglos and:								
	Blacks			Hispanics			Asians		
	1970	1980	Change	1970	1980	Change	1970	1980	Change
<i>Key SMSAs</i>									
Chicago	.919	.878	-.041	.584	.635	.051	.558	.439	-.120
Los Angeles	.910	.811	-.099	.468	.570	.102	.531	.431	-.100
Miami	.851	.778	-.073	.504	.519	.015	.392	.298	-.094
New York	.810	.820	.010	.649	.656	.007	.561	.481	-.080
San Francisco	.801	.717	-.084	.347	.402	.055	.486	.444	-.042
<i>Other SMSAs</i>									
Albany-Schenectady	.677	.617	-.060	.348	.324	-.024	.383	.354	-.030
Albuquerque	.575	.398	-.177	.457	.425	-.032	.340	.305	-.034
Anaheim-Santa Ana	.839	.458	-.381	.320	.416	.096	.274	.249	-.026
Atlanta	.821	.785	-.036	.359	.329	-.031	.458	.291	-.167
Austin	.772	.620	-.152	.507	.441	-.067	.451	.216	-.235
Bakersfield	.834	.644	-.190	.508	.545	.037	.460	.287	-.172
Baltimore	.819	.747	-.072	.442	.381	-.061	.473	.389	-.084
Birmingham	.378	.408	.031	.285	.221	-.065	.379	.261	-.118
Boston	.812	.776	-.036	.486	.579	.093	.499	.474	-.025
Buffalo	.870	.794	-.076	.484	.491	.007	.484	.437	-.047
Cincinnati	.768	.723	-.045	.378	.303	-.075	.433	.330	-.103
Cleveland	.908	.875	-.033	.523	.554	.031	.450	.358	-.093
Columbus	.818	.714	-.105	.441	.330	-.111	.446	.370	-.076
Corpus Christi	.835	.717	-.118	.559	.516	-.042	.525	.297	-.228
Dallas-Ft. Worth	.869	.771	-.098	.425	.478	.052	.439	.291	-.149
Dayton	.869	.780	-.089	.434	.328	-.106	.417	.306	-.111
Denver-Boulder	.876	.684	-.192	.474	.474	.000	.363	.266	-.097
Detroit	.884	.867	-.017	.479	.451	-.029	.461	.375	-.086
El Paso	.528	.347	-.181	.496	.512	.016	.363	.237	-.126
Ft. Lauderdale	.956	.816	-.140	.276	.255	-.022	.449	.318	-.131
Fresno	.784	.624	-.161	.408	.454	.047	.351	.229	-.122
Gary-Hammond	.914	.906	-.008	.591	.562	-.028	.420	.350	-.070
Greensboro-Winston	.654	.560	-.094	.482	.322	-.159	.482	.350	-.131
Houston	.781	.695	-.087	.453	.464	.011	.427	.346	-.081
Indianapolis	.817	.762	-.055	.383	.332	-.051	.402	.360	-.042
Jersey City	.753	.765	.013	.548	.488	-.060	.465	.450	-.015
Kansas City	.874	.789	-.085	.437	.421	-.017	.412	.308	-.105
Louisville	.810	.717	-.092	.389	.271	-.118	.458	.341	-.117
Memphis	.759	.716	-.044	.390	.406	.016	.408	.301	-.107
Milwaukee	.905	.839	-.066	.537	.562	.025	.494	.386	-.108
Minneap.-St. Paul	.856	.683	-.172	.491	.409	-.082	.452	.369	-.083
Nashville-Davidson	.777	.699	-.077	.418	.366	-.052	.521	.388	-.133
Nassau-Suffolk	.744	.755	.011	.291	.362	.070	.422	.345	-.077
New Orleans	.731	.683	-.048	.318	.251	-.068	.459	.427	-.031
Newark	.814	.816	.002	.604	.656	.052	.502	.344	-.158
Norfolk-Va. Beach	.757	.631	-.126	.363	.284	-.079	.519	.347	-.172
Oklahoma City	.911	.709	-.202	.346	.312	-.034	.320	.231	-.089
Paterson-Clifton	.779	.815	.037	.610	.722	.112	.466	.404	-.061
Philadelphia	.795	.788	-.007	.540	.629	.089	.491	.437	-.053
Phoenix	.819	.594	-.225	.484	.494	.009	.441	.328	-.113

Table 3. Continued

Metropolitan Characteristics	Dissimilarity Between Anglos and:								
	Blacks			Hispanics			Asians		
	1970	1980	Change	1970	1980	Change	1970	1980	Change
<i>Other SMSAs—Continued</i>									
Pittsburgh	.750	.727	-.023	.508	.419	-.089	.535	.456	-.079
Portland	.835	.685	-.150	.319	.250	-.069	.330	.271	-.059
Providence-Warwick	.756	.731	-.025	.502	.567	.065	.523	.495	-.028
Riverside-San Ber.	.686	.488	-.197	.373	.364	-.009	.319	.215	-.104
Rochester	.745	.674	-.071	.559	.588	.029	.454	.341	-.113
Sacramento	.688	.559	-.129	.347	.364	.018	.476	.355	-.121
St. Louis	.847	.813	-.034	.354	.339	-.014	.428	.329	-.099
Salt Lake-Ogden	.774	.532	-.242	.362	.307	-.056	.293	.260	-.032
San Antonio	.834	.636	-.199	.591	.572	-.019	.442	.266	-.176
San Diego	.834	.643	-.191	.331	.421	.090	.413	.405	-.009
San Jose	.607	.487	-.120	.402	.445	.043	.254	.295	.041
Seattle-Everett	.819	.682	-.137	.303	.213	-.090	.466	.333	-.133
Tampa-St. Peters	.799	.726	-.073	.560	.484	-.076	.380	.299	-.081
Tucson	.708	.466	-.242	.526	.519	-.007	.526	.365	-.162
Washington, D.C.	.811	.701	-.110	.318	.305	-.013	.365	.268	-.097
Average	.792	.694	-.098	.444	.434	-.010	.437	.342	-.095

Philadelphia, the level of dissimilarity increased or stayed the same. Chicago again displayed the highest level of black segregation, with a dissimilarity index of .878. As before, there is little evidence that blacks in large northern ghettos are achieving residential integration.

Since Hispanic population growth varied considerably across urban areas (data not shown) the pattern of change in residential dissimilarity was highly variable: 33 SMSAs experienced a decline in Hispanic dissimilarity from Anglos, and 27 showed an increase (see Table 3). On average, the increases balanced the declines, so the average level of dissimilarity changed very little over the decade, falling slightly from .444 to .434, and remaining well below the average level of black-Anglo dissimilarity.

As with the exposure probabilities, a closer look at Tables 3 and 4 indicates that trends in Hispanic-Anglo dissimilarity largely reflect patterns of Hispanic immigration over the past decade. The largest Hispanic communities also tended to experience the greatest immigration and the fastest growth over the decade (tabulation not shown). They also recorded the most dramatic increases in spatial dissimilarity. Smaller, slowly growing Hispanic communities experienced little immigration and showed declines in spatial dissimilarity. The former were located primarily in the northeastern and western regions, while the latter were concentrated in the north central and southern regions. Hispanic dissimilarity therefore increased in Los Angeles, Anaheim, and Paterson, and declined in Greensboro, Dayton, and Columbus. Signifi-

cantly, large Hispanic communities that did not experience rapid growth, such as those in New York, Denver, El Paso, Phoenix, Tucson, and San Antonio, showed little or no change in dissimilarity over the decade. Hispanic immigration and population growth thus appear to be the driving forces behind trends in Hispanic-Anglo dissimilarity during the 1970s.

Asians also experienced substantial immigration and rapid population growth over the past decade. In the vast majority of SMSAs, the Asian population at least doubled, and in some case it tripled or quadrupled (data not shown). But with a few exceptions, such as San Francisco, New York, and Los Angeles, there was no recognizable Asian enclave upon which to build. Entering the urban environment, Asians probably sought residences near one another, but there were few Asian neighborhoods in existence, so Asian growth typically entailed entry into Anglo neighborhoods. It is not surprising, therefore, that the trend in Asian residential dissimilarity from 1970 to 1980 was one of universal decline. As Table 3 shows, Asian dissimilarity fell in 59 of 60 cases. In areas where there were very few Asians in 1970, the declines were spectacular: -.235 in Austin and -.228 in Corpus Christi, each of which had only about 1,000 Asians in 1970. Where well-known Asian neighborhoods existed, as in San Francisco, declines were still registered, but they were more modest (-.042).

In 1980, the level of Asian residential dissimilarity varied in a narrow range from .216 to .456. The average value was .342, a remarkably low level of spatial segregation,



Table 4. Average Residential Dissimilarity of Three Minority Groups from Anglos in 60 SMSAs Classified by Selected Metropolitan Characteristics, 1970-1980

Metropolitan Characteristic	Minority Group								
	Blacks			Hispanics			Asians		
	1970	1980	Change	1970	1980	Change	1970	1980	Change
<i>Region</i>									
Northeast	.775	.756	-.019	.511	.540	.029	.482	.418	-.064
North Central	.865	.802	-.063	.469	.436	-.034	.448	.357	-.091
South	.773	.673	-.100	.424	.384	-.040	.435	.308	-.127
West	.774	.592	-.182	.402	.417	.015	.395	.315	-.080
<i>SMSA Population</i>									
Largest	.824	.779	-.045	.699	.608	-.091	.479	.390	-.089
Bigger	.817	.682	-.135	.742	.676	-.066	.400	.315	-.085
Smaller	.776	.680	-.095	.786	.732	-.054	.428	.341	-.087
Smallest	.750	.635	-.116	.610	.554	-.056	.439	.321	-.119
<i>Minority Population</i>									
Largest	.835	.784	-.050	.460	.495	.035	.496	.370	-.069
Bigger	.773	.721	-.053	.476	.481	.005	.408	.316	-.092
Smaller	.814	.698	-.116	.428	.408	-.020	.461	.359	-.102
Smallest	.747	.574	-.173	.414	.353	-.060	.439	.322	-.117
<i>Rate of Population Growth</i>									
Fastest	.766	.587	-.179	.431	.431	.000	.393	.297	-.096
Faster	.763	.648	-.115	.406	.371	-.035	.427	.304	-.123
Slower	.816	.750	-.066	.427	.421	-.006	.448	.370	-.078
Slowest	.823	.791	-.032	.514	.516	.002	.478	.395	-.083
<i>Rate of Minority Immigration</i>									
Highest	.782	.678	-.104	.490	.521	.031	.429	.315	-.114
Higher	.767	.652	-.115	.438	.376	-.062	.438	.345	-.093
Slower	.823	.722	-.101	.419	.404	-.015	.445	.345	-.100
Slowest	.795	.724	-.071	.431	.436	.005	.434	.362	-.072
<b>Average</b>	<b>.792</b>	<b>.694</b>	<b>-.098</b>	<b>.444</b>	<b>.434</b>	<b>-.010</b>	<b>.437</b>	<b>.342</b>	<b>-.095</b>

Note: Number of SMSAs in different regions are as follows: Northeast (14), North Central (11), South (19), West (16). The remaining variables are classified into equal quartiles of 15 SMSAs each.

roughly equivalent to the 1970 levels for the "old" European ethnic groups (British, German, Irish, Scandinavian) (Massey 1985). If Asian migration continues, however, this level will most likely increase as Asian enclaves emerge and become poles of attraction for new immigrants. The fact that over the 1970s Asian spatial isolation increased (Table 1) while dissimilarity fell (Table 3) suggests that many areas of recent Asian settlement contain the seeds of new enclaves. But at this time, the level of Asian segregation remains very low. Even in San Francisco, Asian-Anglo dissimilarity was only .444, half the level of the highest black-Anglo dissimilarity index (in Chicago) and two-thirds the highest Hispanic-Anglo index (in New York).

#### EXPLAINING THE PATTERNS

The foregoing section has shown that, despite recent declines in some metropolitan areas, the level of black segregation remains quite high, especially in cities where the majority of urban blacks live. Hispanic segregation is relatively moderate, although it has increased substantially

in areas of rapid immigration. Asian segregation is very low, even in cities with large Asian populations. Behind these generalizations, however, lies considerable variability, particularly among Hispanics and blacks. Dissimilarity indices for blacks ranged from .408 to .878 in 1980, while those for Hispanics varied from .213 to .656. Similarly, black-Anglo interaction probabilities ranged from .125 to .815, while Hispanic-Anglo probabilities varied from .229 to .910.

In this section we attempt to account for interurban variation in segregation by estimating statistical models derived from a theoretical perspective developed in earlier research (Massey and Muillan 1984; Massey and Denton 1985; Massey 1985). The perspective argues that spatial assimilation is driven by social mobility and, among immigrant groups, by acculturation, and that these variables affect spatial integration through the intervening step of suburbanization. Because these processes are strongly conditioned by the structural context provided by specific urban environments, metropolitan-level characteristics must be explicitly controlled.

As a minority group's socioeconomic status increases, its members seek to improve their spatial position in urban society, which typically involves moving into neighborhoods with greater prestige, more amenities, safer streets, better schools, and higher-value homes (Massey et al. forthcoming). Accomplishing these goals usually brings a minority group into greater spatial contact with majority members and promotes residential integration. Among immigrant-origin populations such as Hispanics and Asians, moreover, acculturation increases the desire of minority members to live in neighborhoods where majority members predominate (acculturation is not relevant to the case of American blacks, of course). Both socioeconomic mobility and acculturation reduce the social distance between minority members and native whites, so the former's entry into a neighborhood does not spark hostility, resistance, and systematic out-migration by the latter. Over time, therefore, social mobility and acculturation bring about the spatial assimilation of minority groups in urban society.

Although changes in socioeconomic status and acculturation may lead directly to spatial assimilation, in the United States an important intervening process is suburbanization. In post-war America, the process of racial and ethnic integration has been inextricably bound up with movement to the suburbs, where levels of segregation are generally lower and Anglos predominate (Golant and Jacobsen 1978). To the extent that suburban residence may be precluded for some minority groups because of discriminatory housing practices, an important avenue of residential integration may be closed off.

Processes of social mobility and acculturation are strongly affected by the larger structural context of urban society (Massey 1985). An important structural element is the housing market. During periods of rapid home construction and residential expansion, such as occurred between 1945 and 1970, residential mobility is accelerated and the process of spatial assimilation encouraged. A second factor is the state of the urban economy, which acts as a structural constraint on social mobility and, hence, spatial assimilation. During periods of economic growth, widespread social mobility generates demand for improved housing in more desirable, typically suburban, neighborhoods and leads to considerable residential mobility, which in turn facilitates integration. A third contextual element is the history of immigration into the urban area. During periods of rapid immigration, social networks channel minority members into existing areas of minority settlement, raising the overall level of segregation if the ethnic enclave can accommodate the newcomers, and lowering

it when growth spills over into surrounding Anglo neighborhoods (Taeuber and Taeuber 1965). The arrival of new immigrants also lowers the average level of acculturation within the ethnic group, dampening motivations for mobility and integration. Finally, any analysis of segregation must control for the physical stock of the city. Cities built up before the Second World War have ecological structures that are more conducive to segregation (Hershberg et al. 1981), with densely settled cores and tightly packed working-class neighborhoods clustered around old factories.

Our analytic strategy is to operationalize this simple model, using information available from the census, and to estimate OLS regression models in an effort to account for interurban variation in black, Hispanic, and Asian segregation in 1980. Three dependent variables are considered: the proportion of group members living in the suburbs, the probability of contact with Anglos (from Table 1), and residential dissimilarity from Anglos (from Table 3). Because all three variables have a limited range, we transform them into logits before conducting the regression analyses. Indicators of overall acculturation and socioeconomic status are computed for each of the three minority groups and regressed, across metropolitan areas, on their respective segregation measures, controlling for metropolitan structure. Minority populations with high average levels of acculturation and socioeconomic status are hypothesized to have higher probabilities of Anglo contact, lower levels of dissimilarity from Anglos, and higher proportions living in the suburbs.

Acculturation is operationalized by computing the proportion of minority members who report speaking English well, and the proportion of minority members who are native born. Since 98 percent of blacks are born in the United States and are native English speakers, acculturation is not relevant for them and these variables are not employed in the black models. Socioeconomic status is measured in both absolute and relative terms. Absolute status is measured by median family income and relative status by the degree of occupational dissimilarity between minority members and Anglos. Income provides economic resources that make residential mobility possible, and rising occupational status decreases the minority group's social distance from Anglos, thereby reducing their potential threat and making entry into Anglo neighborhoods less problematic. We therefore expect the probability of Anglo contact to be a positive function of income and a negative function of occupational dissimilarity; we also expect residential dissimilarity to be negatively related to income and positively related to occupational dissimilarity. Educational variables were not

included in the equations because they were found to be highly collinear with the other two indicators.

Structural context is measured by five indices computed at the metropolitan level. The state of the housing market is indicated by the average annual rate of growth in the median value of housing between 1970 and 1980, with a high rate of inflation indicating a tight market and a relative shortage of housing. The state of the economy is indicated by the annual rate of growth in metropolitan employment between 1970 and 1980. The relative age of a metropolitan area is measured by the median age of the housing in it. Recent trends in immigration are measured by the average annual growth rate of the foreign-born population within each minority group, and relative population growth is measured by computing the minority-Anglo growth differential (minority rate minus Anglo rate). Positive values of the latter variable indicate a predominance of minority over Anglo growth, and negative values indicate the opposite. "Minority" refers to the group in question (blacks, Hispanics, or Asians), rather than the three groups added together.

Three other factors are taken into account in the statistical models. First, we control for the relative proportion of minority members in the metropolitan area. Population composition is mathematically related to the likelihood of contact with Anglos, and the relative number of minority members has strong implications for spatial behavior generally. Second, we control for the effect of ethnic/racial composition *within* the minority groups themselves, measuring the proportion of Hispanics who are Mexican, Cuban, or Puerto Rican, and the proportion of Asians who are Japanese, Chinese, Korean, Vietnamese, or Indian. We also control for the proportion of Hispanics who are black, since prior work has shown this to be an important variable affecting Hispanic segregation (Massey and Bitterman 1985).

Finally, we make an adjustment for sample selectivity. Of the 318 SMSAs defined by the Census Bureau in 1980, our sample includes only the 50 largest plus 10 other areas with sizeable Hispanic populations. Since these metropolitan areas are highly selected, estimates of slope parameters will be biased unless a correction is made (Berk 1983). We employed the method of Olsen (1980), which uses OLS to estimate a selection equation that predicts the likelihood of an SMSA's inclusion in the sample ( $P$ ) from an instrumental variable, and then uses the term  $P-1$  as a control to eliminate the effect of sample selectivity from the final equation estimates. Specifically, we regressed the log of SMSA size on a 0-1 variable that equalled 1 when the SMSA was included in our sample and

0 otherwise. (The proportion Hispanic could not also be used as a predictor in this selection equation because it appears in the regression equations we seek to correct.) The estimated selection equation was  $P = -3.680 + .308 \cdot \log(\text{SMSA size})$ , estimated over 318 SMSAs with an adjusted  $r^2$  of .593.

The analysis proceeds in three phases. First we analyze the process of minority suburbanization, then consider the impact of socioeconomic, cultural, and structural variables on Anglo-interaction probabilities, and, finally, measure the effect of these variables on spatial dissimilarity from Anglos. The last two steps control for the level of suburbanization as a predetermined variable.

Table 5 begins the analysis by examining the determinants of minority suburbanization for blacks, Hispanics, and Asians in 59 metropolitan areas (Nassau-Suffolk has no central city and therefore no suburbs). Because the vast majority of blacks are native born, measures of acculturation were not included for this group. None of the socioeconomic or metropolitan structural variables has a significant effect on the level of black suburbanization, and no variance is explained by the model. The intercept corresponds to a very low level of suburbanization, about .150, and, in fact, the average in the 59 SMSAs was only .282 (compared to .482 for Hispanics and .530 for Asians.) Neither socioeconomic status nor metropolitan context influences the level of black suburbanization, which remains quite limited.

In contrast, interurban variation in Hispanic and Asian suburbanization is highly related to socioeconomic status. Consistent with expectations, lower Hispanic-Anglo occupational dissimilarities and higher Hispanic incomes are associated with greater suburbanization. Asian suburbanization is likewise positively related to income and the relative number of native born: as income and the proportion of natives rise, so does the relative number of Asians living in suburbs. Table 5 contains one coefficient that is opposite the expected direction. English language ability among Hispanics is *negatively* related to suburbanization, a finding that is difficult to interpret.

Suburbanization of Hispanics and Asians is apparently not affected by the ethnic composition of these groups, but is influenced by elements of metropolitan context. Hispanic suburbanization is greater in areas with a relative scarcity of housing (see the coefficient for housing inflation), while Asian suburbanization is lower in areas that experienced rapid economic growth (see the coefficient for labor force growth). Both of these findings are somewhat anomalous, since tighter housing markets normally restrict residential mobility

Table 5. Logistic Regression of Selected Variables on the Proportion of Blacks, Hispanics, and Asians Living in Suburban Areas of 59 Metropolitan Areas

Independent Variables	Minority Group					
	Blacks		Hispanics		Asians	
	B	SE	B	SE	B	SE
<i>Level of Acculturation</i>						
% Native born	—	—	2.008	2.504	7.029*	4.274
% Speaking English well	—	—	-8.144*	4.443	5.995	4.642
<i>Socioeconomic Status</i>						
Median family income	-0.069	0.104	0.100*	0.606	0.101*	0.606
Occup. dis. from anglos	3.814	4.797	-7.912**	3.847	-0.635	3.370
<i>Hispanic Composition</i>						
% Black Hispanics	—	—	1.398	1.971	—	—
% Mexican	—	—	-1.207	0.976	—	—
% Cuban	—	—	-0.500	2.037	—	—
% Puerto Rican	—	—	-1.306	1.625	—	—
<i>Asian Composition</i>						
% Japanese	—	—	—	—	-3.721	4.948
% Chinese	—	—	—	—	1.118	2.242
% Korean	—	—	—	—	0.396	4.076
% Vietnamese	—	—	—	—	-2.323	5.371
% Indian	—	—	—	—	1.277	3.874
<i>Metropolitan Context</i>						
Housing inflation rate	9.650	6.534	12.418**	5.177	7.697	9.337
Employment growth rate	-4.368	13.298	-9.512	12.750	-23.184*	14.101
Growth rate of for. born	-0.211	3.017	-4.269*	2.595	2.823	2.931
Anglo growth differential	9.719	10.963	4.730	9.094	6.641	6.532
Median age of housing	-0.028	0.031	-0.001	0.034	-0.038	0.028
Group's % of population	-0.041	2.534	-3.360**	1.547	-15.318	15.001
<i>Selectivity Correction</i>						
P-1	-0.380	0.763	-1.648**	0.625	0.693	0.800
Intercept	-1.697	2.249	6.235	4.373	-8.605**	3.911
Adjusted R <sup>2</sup>	0.000		0.301**		0.162**	
N	59		59		59	

Note: The Nassau-Suffolk SMSA has no central city.

\*  $p < .10$ .

\*\*  $p < .05$ .

and suburbanization, while economic growth usually promotes integration by fostering group mobility. Consistent with earlier reasoning, however, the level of Hispanic suburbanization is lower in areas that experienced higher immigration over the prior decade, and in areas with a high proportion of Hispanics.

In spite of a few anomalous coefficients in the Hispanic and Asian models, results consistently show that blacks are highly disadvantaged in the suburbanization process. Not only are levels of black suburbanization quite low, but they are unrelated to any of the explanatory variables we examined. Hispanic and Asian suburbanization, in contrast, are explained largely by objective indicators of socioeconomic status and acculturation, and they are also related to structural trends within metropolitan areas. With rising socioeconomic status, progressive suburbanization of these two groups should occur; their levels of suburbanization are already considerably above that of blacks.

Table 6 carries the analysis of spatial assimilation a step further by considering the effect of acculturation and socioeconomic status

on Anglo interaction probabilities, controlling for the level of suburbanization, which is predetermined. The proportion of variance explained by the black model is roughly half that explained by the Hispanic model, but about the same as the Asian model. The probability of black contact with Anglos is determined principally by relative occupational dissimilarity from Anglos and by the proportion of blacks in the metropolitan area. In other words, there is some evidence of a process of spatial assimilation among blacks: as social distance from Anglos decreases, the likelihood of residential contact with them increases. But black suburbanization is unrelated to the probability of Anglo contact. To the extent that spatial assimilation occurs, therefore, it happens independently of suburbanization, primarily through occupational mobility rather than spatial mobility to the suburbs.

Hispanic-Anglo contact, in contrast, is highly related to suburbanization as well as acculturation and socioeconomic status. Hispanic spatial assimilation is promoted strongly by English language ability, as one would expect, but is inversely related to income and the proportion

Table 6. Logistic Regression of Selected Variables on the Probability of Hispanic, Black, and Asian Contact with Anglos in 60 SMSAs

Independent Variables	Minority Group					
	Blacks		Hispanics		Asians	
	B	SE	B	SE	B	SE
<i>Level of Acculturation</i>						
% Native born	—	—	-2.338**	0.927	-1.107	2.354
% Speaking English well	—	—	8.458**	1.695	-3.968	2.519
<i>Socioeconomic Status</i>						
Median family income	-0.043	0.055	-0.045**	0.023	-0.007	0.033
Occup. dis. from anglos	-5.093**	2.567	0.423	1.493	2.792	1.795
<i>Level of Suburbanization</i>						
% of group in suburbs	0.077	0.430	0.699**	0.264	0.792**	0.399
<i>Hispanic Composition</i>						
% Black Hispanics	—	—	-5.124**	0.732	—	—
% Mexicans	—	—	0.168	0.362	—	—
% Cubans	—	—	0.277	0.751	—	—
% Puerto Ricans	—	—	-0.088	0.603	—	—
<i>Asian Composition</i>						
% Japanese	—	—	—	—	0.617	2.651
% Chinese	—	—	—	—	-2.206*	1.197
% Korean	—	—	—	—	-0.572	2.172
% Vietnamese	—	—	—	—	4.215	2.870
% Indian	—	—	—	—	1.752	2.066
<i>Metropolitan Context</i>						
Housing inflation rate	-1.836	3.512	-1.870	2.014	-2.695	5.029
Employment growth rate	2.029	7.018	1.334	4.719	-8.039	7.741
Growth rate of for. born	0.510	1.587	0.924	0.986	-2.833*	1.581
Anglo growth differential	4.265	5.825	7.554**	3.354	-8.123**	3.518
Median age of housing	-0.023	0.016	0.010	0.013	-0.021	0.015
Group's % of population	-2.390*	1.333	-3.166**	0.590	-0.780	8.114
<i>Selectivity Correction</i>						
P-1	-0.549	0.402	-0.245	0.244	-0.020	0.431
Intercept	1.672	1.182	-4.509**	1.685	5.998**	2.148
Adjusted R <sup>2</sup>	0.357**		0.876**		0.360**	
N	60		60		60	

\*  $p < .10$ . \*\*  $p < .05$ .

native born, contrary to predictions. The latter anomalous relationships are difficult to explain, but perhaps reflect the fact that suburbanization has been controlled. If movement to the suburbs is the primary channel of spatial assimilation, then SES or acculturation might be unrelated or even negatively related to Anglo contact after its effect is removed.

Contextual variables also affect the likelihood of Hispanic contact with Anglos, being greater in areas where Hispanic growth dominates over that of Anglos. Rapidly growing Hispanic populations cannot always be accommodated within existing Hispanic enclaves, and spillover into adjacent Anglo areas increases residential contact between the two groups. The probability of Anglo interaction is also strongly affected by the racial composition of Hispanics, being markedly lower in SMSAs where a large share of Hispanics are black, underscoring the salience of race in American society.

The Asian model presents a somewhat different vision of spatial assimilation. Asian-Anglo contact is not related to indicators of SES or acculturation, but is affected by several

contextual variables, and by the relative number of Chinese. In contrast to the case of Hispanics, high rates of Asian population growth and immigration reduce the likelihood of contact with Anglos. Since Asians are not likely to have saturated enclaves in most SMSAs, spillover into Anglo areas does not occur. Rapid growth through immigration in this case promotes the formation of new enclaves through chain migration, thereby reducing contact with Anglos. The Chinese, being the largest and oldest Asian group, tend to have more established enclaves "Chinatowns"), so the proportion of Chinese is negatively associated with Anglo contact.

In both the Asian and Hispanic models, suburbanization is very strongly related to the probability of Anglo contact. It is an endogenous variable predetermined by SES and acculturation. Thus, although the *direct* effects of acculturation and SES on Anglo contact may be weak and inconsistent, these variables have important *indirect* effects through their impact on the likelihood of suburban residence. For Asians and Hispanics, suburbanization is a key

step in the larger process of spatial assimilation, one that is largely closed to blacks.

The importance of suburbanization in the process of desegregation is reaffirmed by the results of Table 7, which considers the determinants of residential dissimilarity. Indicators of acculturation and SES are again weakly (in the case of Asians) or inconsistently (in the case of Hispanics) related to segregation, but suburbanization is strongly related to dissimilarity for both groups. As before, suburbanization has no impact on the level of black-white segregation.

The black model explains significantly less variance than the Hispanic or Asian models, with only three substantive variables being marginally significant. The rate of employment growth and the rate of black population increase are both negatively associated with dissimilarity from Anglos. In other words, metropolitan areas with slow economic growth and growing black populations (older industrial cities with large black ghettos) are associated with a high degree of residential segregation. Contrary to expecta-

tions, rising black income produced a significant increase, rather than a decrease, in segregation.

As before, results for Hispanics present an inconsistent picture of spatial assimilation, once suburbanization is controlled. Hispanic segregation falls with rising English language ability, as expected, but rises with an increasing prevalence of the native born, contrary to theoretical predictions. Rapid Hispanic population growth promotes lower levels of segregation, probably through the spillover effect noted earlier. Among Asians, neither acculturation nor SES affects residential dissimilarity once suburbanization is controlled. Anglo-Asian dissimilarity is affected primarily by contextual and compositional factors, with segregation being greater in tighter housing markets and older SMSAs, and being markedly lower among Asian populations with a relatively large number of Japanese.

Taken together, the last three tables suggest that the spatial assimilation of Hispanics and Asians occurs primarily through movement to the suburbs. Indicators of acculturation and assimilation are strongly related to suburbanization for both groups, but once suburbanization is

Table 7. Logistic Regression of Selected Variables on the Residential Dissimilarity of Blacks, Hispanics, and Asians from Anglos in 60 SMSAs

Independent Variables	Minority Group					
	Blacks		Hispanics		Asians	
	B	SE	B	SE	B	SE
<i>Level of Acculturation</i>						
% Native born	—	—	2.139**	0.758	0.742	0.947
% Speaking English well	—	—	-4.923**	1.386	-0.003	1.013
<i>Socioeconomic Status</i>						
Median family income	0.076*	0.044	0.010	0.018	0.004	0.013
Occup. dis. from anglos	2.844	2.049	1.693	1.220	-0.847	0.723
<i>Level of Suburbanization</i>						
% of group in suburbs	-0.258	0.343	-0.577**	0.216	-0.416**	0.161
<i>Hispanic Composition</i>						
% Black Hispanics	—	—	0.617	0.598	—	—
% Mexicans	—	—	-0.047	0.296	—	—
% Cubans	—	—	0.509	0.614	—	—
% Puerto Ricans	—	—	-0.219	0.493	—	—
<i>Asian Composition</i>						
% Japanese	—	—	—	—	-2.245**	1.066
% Chinese	—	—	—	—	0.316	0.481
% Korean	—	—	—	—	0.132	0.873
% Vietnamese	—	—	—	—	-0.711	1.154
% Indian	—	—	—	—	0.769	0.831
<i>Metropolitan Context</i>						
Housing inflation rate	-1.230	2.803	-0.356	1.646	4.985**	2.022
Employment growth rate	-9.443*	5.601	-4.876	3.857	-3.088	3.113
Growth rate of for. born	0.252	1.267	0.332	0.806	0.928	0.636
Anglo growth differential	-8.326*	4.650	-5.427**	2.742	-2.164	1.415
Median age of housing	0.020	0.013	0.004	0.010	0.022**	0.006
Group's % of population	-0.183	1.063	-0.120	0.482	-3.984	3.263
<i>Selectivity Correction</i>						
P-1	0.804**	0.321	0.231	0.199	0.366**	0.173
Intercept	-0.194	0.944	2.226	1.377	-1.162	0.864
Adjusted R <sup>2</sup>	0.445		0.786		0.593	
N	60		60		60	

\*  $p < .10$ . \*\*  $p < .05$ .

controlled, they are weakly and inconsistently related to indicators of segregation. For blacks, this avenue to residential desegregation appears to be closed. None of the variables we considered are related to black suburbanization, and the level of black suburbanization is, in turn, unrelated to either measure of residential segregation.

Suburbanization is, therefore, a key factor in the spatial assimilation of all three groups. For Hispanics and Asians, ongoing processes of suburbanization have generated low-to-moderate levels of residential segregation that reflect underlying socioeconomic differences and structural shifts. For blacks, significant barriers to suburban settlement perpetuate high levels of residential segregation that are resistant to socioeconomic or structural effects.

## DISCUSSION

Our results indicate that blacks, Hispanics, and Asians occupy very different positions in urban society. The contrast in the spatial situations of the groups is well-illustrated by a quick look at San Francisco, where each group represents about 11 percent of the population. In 1980, the probability of black contact with Anglos was .299, compared to .582 for Hispanics and .564 for Asians; and black-Anglo dissimilarity stood at .717, with respective figures of .444 and .402 for Asians and Hispanics. In other words, given the same relative numbers, blacks are nearly twice as segregated as Hispanics or Asians.

Our analyses suggest several conclusions about race and residence in U.S. cities. First, there has been remarkably little change in the status quo since 1970. Articles that contrasted patterns of Hispanic and black segregation using 1970 census data arrived at nearly the same conclusions as we did. In both 1970 and 1980 there is little evidence of a significant process of spatial integration among blacks in large metropolitan areas. The level of black segregation is not strongly related to indicators of socioeconomic status, SES is unrelated to black suburbanization, and the level of black suburbanization has no influence on segregation. In short, a key step in the process of spatial assimilation for other groups, suburbanization, plays no role in black integration. Some blacks may be moving to suburban areas, but this movement does not seem to be related to their socioeconomic characteristics, and it has had no measurable impact on the overall level of black segregation. Either blacks are moving to suburbs in numbers too small to make a difference, or suburbs and central cities are equally segregated.

A second conclusion is that the forces of

racial change that transformed American society during the 1970s have had a marginal impact on the spatial behavior of blacks and whites in American cities. Despite the advent of fair housing legislation, more tolerant white racial attitudes, and a growing black middle class with income sufficient to promote residential mobility, the segregation of blacks in large cities hardly changed. If the black middle class has abandoned the black poor, it has not been by moving to Anglo neighborhoods, at least on a significant scale. Most blacks continue to reside in predominantly black neighborhoods, even in cities with relatively large and affluent black middle classes, such as New York, Chicago, and Philadelphia.

The patterns of segregation we have described also speak to the meaning of race in American society. The high degree of black residential segregation, and its relative imperviousness to socioeconomic influences, suggest that race continues to be a fundamental cleavage in American society. Yet it is not race *per se*. Asians are also members of nonwhite racial groups, easily identifiable as such by Anglo whites, but there is little evidence that Anglos harbor significant prejudice against them when it comes to sharing urban residential space. Asians are characterized by very low levels of residential segregation, even in urban areas where they particularly concentrate. It is not race that matters, but black race.

This fact is underscored by the case of black Hispanics. We found that a relatively large number of black Hispanics was significant in reducing the likelihood of contact with Anglos. Other research has shown that black Hispanics are highly segregated from other groups, including white Hispanics (Goldstein and White 1985; Massey and Mullan 1985; Massey and Bitterman 1985). Blacks are apparently viewed by white Americans as qualitatively different and, by implication, less desirable as neighbors, than members of other racial or ethnic groups.

For blacks seeking integration into the mainstream of American society, the issue of race is still very much alive. Blacks may have won political freedom, and may have made substantial progress in attaining their economic goals, but they have yet not achieved the freedom to live wherever they want. If black residential integration has occurred at all, it has not been through residential mobility within metropolitan areas where the vast majority of blacks live, but through movement to small and mid-sized cities that presently contain few black residents. Perhaps the growth of black populations in these smaller metropolitan areas will be the means by which residential integration will finally occur in the United States.

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# RACE, FAMILY STRUCTURE, AND DELINQUENCY: A TEST OF DIFFERENTIAL ASSOCIATION AND SOCIAL CONTROL THEORIES\*

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*Studies of the relationship between race and delinquency have typically found that broken homes lead to greater delinquency among blacks than whites, but have not demonstrated empirically why this is so. This paper derives theoretical mechanisms from differential association theory and social control theory, specifying how broken homes may influence delinquency among both blacks and nonblacks. The analysis specifies a structural equation model of delinquency (Matsueda 1982), derives competing hypotheses from the two theories, and estimates a cross-population model for blacks and nonblacks using data from the Richmond Youth Project. Consistent with previous research, we find that broken homes have a larger impact on delinquency among blacks than nonblacks, but, unlike previous studies, our model explains this effect completely. In both populations, the effects of broken homes and attachment to parents and peers are mediated by the learning of definitions of delinquency, a finding that supports differential association over social control theory.*

Although race is a critical variable in many theories of crime, little empirical research has examined competing explanations of the race-delinquency relationship. There are perhaps three reasons for this. First, given the history of racial discrimination in the United States, any examination of black-white differences in unlawful behavior is likely to be politically sensitive and controversial (Wilson and Herrnstein 1985; Wilson 1985). Second, differences in criminal and delinquent behavior, as measured by official statistics, have been attributed to racial bias in the criminal justice system. Third, racial disparities in delinquency have been difficult to measure reliably. Indeed, researchers disagree over the extent to which rates of unlawful behavior vary by race: official statistics and victimization surveys show wide disparities, while self-report surveys show few differences (Hindelang 1978; Hindelang, Hirschi, and Weis 1979, 1981). Moreover, because the responses of blacks to survey questions contain more random variability than those of whites, some have cautioned against making racial compar-

isons with delinquency data (Hirschi 1969). This implies that any cross-race comparison must consider differential errors of measurement (Bielby, Hauser, and Featherman 1977).

Most previous research on black-white differences in delinquency has focused on the structure of the family. Stimulated by the Moynihan Report (1965), which hypothesized that black youths commit more delinquent acts in part because of a tangle of pathology originating in female-headed households, unemployment, illegitimacy, and differential socialization, such research has examined the joint relationships among race, broken homes, and delinquency. The conclusions have been mixed: most researchers find that broken homes have a larger effect on delinquency among blacks (Monahan 1957; Moynihan 1965; Rosen, Lalli, and Savitz 1975); some find a greater effect among whites (Toby 1957; Chilton and Markle 1972; Austin 1978); still others find little difference by race (Tennyson 1967; Berger and Simon 1974). This literature has been preoccupied with the demographic question of whether the effect of broken homes on delinquency varies by race. From a theoretical standpoint, a more significant question concerns the causal mechanisms intervening between broken homes and delinquency for both races. What is needed, then, is a theoretical model that can explain these relationships.

This paper examines delinquent behavior among blacks and nonblacks using a causal model derived from two dominant sociological theories of delinquency: differential association theory and social control theory. The model builds on a statistical model previously esti-

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mated to test differential association against control theory (Matsueda 1982). We use the model to examine differences in parameters across populations of black and nonblack youth, to focus on the relationship between family structure and delinquency, and to test the efficacy of differential association versus social control theory across race. The first section discusses the implications of differential association and social control theories for explaining the relationships among race, broken homes, and delinquency. Here we derive several testable hypotheses from the competing theories. The second section presents a structural equation model of these relationships, estimates the model's parameters, and tests key hypotheses. The third section discusses the implications of the results for theorizing about race, social structure, and delinquency.

#### MODELING RACIAL DIFFERENCES: DIFFERENTIAL ASSOCIATION VERSUS SOCIAL CONTROL THEORY

Our task is to develop a social-psychological explanation of the joint relationships among race, broken homes, and delinquency. Two distinct mechanisms can explain such relationships. First, race and broken homes could interact in their effects on delinquency: the effect of broken homes and other determinants of delinquency could be greater among blacks. Second, race could influence delinquency indirectly through its effects on broken homes. The latter assumes that the effect of broken homes on delinquency does not vary by race; consequently, testing the interaction effect is logically prior. For this reason, and because prior studies suggest that both measurement and substantive processes vary by race, we will examine a cross-race model of delinquency. Previous research suggests that the effects of race and broken homes must be disentangled from the influences of socioeconomic status and neighborhood processes (Shaw and McKay 1969; Monahan 1957; Moynihan 1965; Berger and Simon 1974). Therefore, we need to locate those intervening social-psychological processes explaining such relationships.

According to Sutherland's (1947) theory of differential association, delinquency is rooted in normative conflict. Modern industrial societies contain conflicting structures of norms, behavior patterns, and definitions of appropriate behavior that give rise to high rates of crime. At the *group* level of explanation, Sutherland posited that normative conflict is translated into group rates of delinquency through differential social organization: the extent to which a group is organized for or against delinquency determines its rate of law violation. This differential

organization consists of neighborhood organization, family processes, peer relationships, and the distribution of age, race, and class.

At the *individual* level, Sutherland maintained that normative conflict is translated into individual acts of delinquency through differential association. Definitions favorable and unfavorable to delinquent behavior are learned through communication, primarily in intimate groups. Whether delinquency occurs depends on the ratio of learned definitions favorable and unfavorable to that act. Moreover, each definition is weighed by four modalities: frequency, duration, priority, and intensity. Definitions presented more frequently, for a longer time, earlier in life, and from a more prestigious source receive more weight.

Taken together, the individual and group components of differential association explain the organizational and learning mechanisms by which race and family status influence delinquent behavior. The learning mechanism (differential association process) should be invariant across race, although the context or source of that learning, such as parents, peers, or neighborhoods (differential social organization), may vary by race. For example, if a broken home impedes parental supervision and attachment, it could indirectly increase a child's contact with prodelinquent definitions from delinquent boys and other influences outside the home (Sutherland and Cressey 1978, p. 219-24; Shaw and McKay 1931). Furthermore, broken homes may hamper the formation of attachments to parents (prestige) and the transmission of antidelinquent definitions from parent to child; thus, the prodelinquent organization of the community or neighborhood would not be offset by antidelinquent influences within the home. Since racial segregation often limits blacks to inner-city neighborhoods with low socioeconomic status and abundant definitions favorable to street crimes (Sutherland and Cressey 1978, p. 220), the influence of broken homes on delinquency may be particularly acute for blacks. The important point is that for both blacks and nonblacks, structural variables such as broken homes and neighborhood organization affect delinquency by influencing the dynamic process of learning definitions favorable and unfavorable to crime.

In contrast to differential association, Hirschi's (1969) social control theory denies the existence of normative conflict and ignores the importance of motives for delinquency, such as prodelinquent definitions. Control theory posits a single conventional moral order in society and assumes that the motivation for delinquency is invariant across persons. The question is not, "Why do some people violate the law?" since we are all equally motivated to do so, but rather,

"Why do most people refrain from law violation?" Hirschi's answer is that they are dissuaded by strong bonds to conventional society: attachment, commitment, involvement, and belief.

Attachment to others dissuades persons from delinquency through a moral process: those with warm relationships with their parents or friends are likely to consider their reactions to the unlawful act. Because only a single moral order exists, that reaction will always be negative. Commitment to conventional lines of action reflects an investment of time and energy in procuring an education, developing a business, or building a virtuous reputation. The greater the investment, the less likely the person will jeopardize it by violating the law. Involvement in conventional activities simply limits one's time to contemplate and execute illegal acts. Finally, belief in the moral order directly taps an individual's internalization of conventional morality. Here, Hirschi reconceptualizes Sutherland's definitions of delinquency to conform to the assumptions of control theory: since there is only one moral order, beliefs concerning delinquency are all conventional, and the greater the belief the less likely the deviation.

Each of these components of the bond, while intercorrelated, are said to affect delinquency independently and additively (Hirschi 1969, pp. 27-30). While differential association theory implies that attachments, involvements, and commitments will affect delinquency only indirectly through their effects on definitions (belief), control theory maintains that each element of the bond *itself* affects delinquency directly (Jensen 1972; Kornhauser 1978; Matsueda 1982).

Control theory implies that the causes of delinquency (social bonding) are the same for all racial groups (Hirschi 1969, p. 80). The theory would receive strong support if the absolute effect on delinquency of each element of the bond were identical for all races. This would imply that the theory describes a deep invariant structure that persists in the face of racial segregation and discrimination. But confirming control theory may not require such invariance, instead requiring only that the elements of the bond explain the probability of delinquent behavior. Thus, we might expect socialization practices or belief systems to vary across racial groups, causing attachment, commitment, involvement, and belief to affect delinquency differently by race.

Furthermore, the relative strength of structural determinants of social bonding may also vary by race. Here, we are on less-solid ground, since Hirschi (1969, p. 113) had little to say about factors affecting the strength of elements of the bond. Nevertheless, if we conceptualize the struc-

tural-level counterpart of bonding as social disorganization—a community's inability to control the behavior of juveniles because of weak and unlinked institutions—we can hypothesize about racial differences in bonding and its determinants (Kornhauser 1978; Shaw and McKay 1969). Broken homes, lower socioeconomic classes, and high-crime neighborhoods (disorganization) should influence delinquency by impeding the formation of strong attachments, commitments, involvements, and beliefs. Because nonintact homes undermine parent-child relations, attachment to parents—perhaps the most important element of the bond—should be the principal intervening variable between broken homes and delinquency (Hirschi 1969, 1983).<sup>1</sup> In turn, attachments to parents should generalize, allowing attachments to form among peers and reinforcing strong moral beliefs. If Moynihan and others are correct that blacks are ensnared in a tangle of pathology, then social control theory would claim that this pathology is a reflection of disorganization and that broken homes, social class, and neighborhood delinquency will produce more delinquency among blacks by inhibiting the formation of strong attachments and beliefs.

In sum, control theory and differential association make different predictions of the causes of delinquency among black and nonblack males. Social control theory predicts that, for both blacks and nonblacks, delinquency is determined by the independent effects of the elements of the social bond. Family structure may affect the elements of the social bond differently across race, but each element of the bond should exert a unique effect on delinquency for both races. The relative importance of these bonds, however, may vary across race, due to a different emphasis on socialization practices, which in turn stems from social disorganization. Differential association, however, predicts that, for both blacks and nonblacks, delinquency is determined by learning definitions of the legal code (beliefs), which mediate the influence of attachments, commitments, and involvements. The sources of that learning, however, are determined by individuals' group location in the social structure, which organizes their patterns of interactions, and which may differ by race. This implies that the determinants of a person's learned definitions, such as being from a broken home, a trouble-ridden neighborhood, a close family unit, or a delinquent peer

<sup>1</sup> Hirschi (1969, 1983) has argued that single-parent families should have similar rates of delinquency as intact families, since, all things being equal, one parent should be as effective as two in socializing children. Nevertheless, all things are never equal, and logically, for social control theory, if broken homes influence delinquency, they do so by attenuating the elements of the social bond.

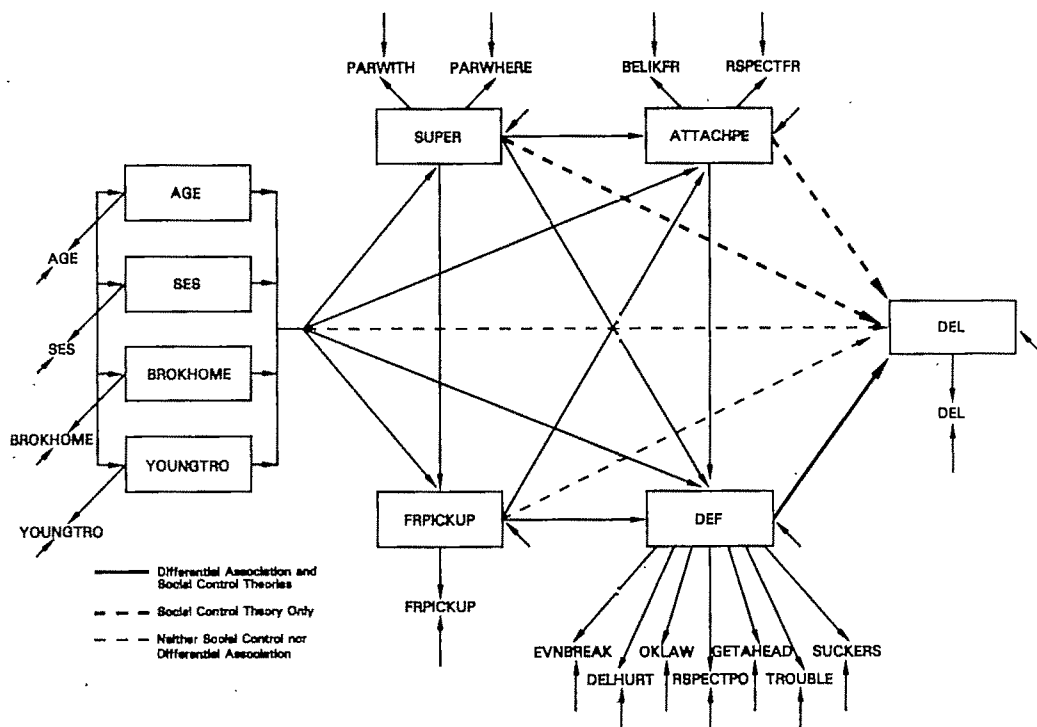


Fig. 1. Path Diagram of the Full Structural Equation Model of Delinquency

group, may vary across race, but the determinant of delinquency—an excess of definitions favorable to delinquency—will not.

#### A CROSS POPULATION MODEL OF RACE AND DELINQUENCY

Our investigation analyzes Matsueda's (1982) causal model of differential association, control theory, and delinquency by replicating the model on the black population of the Richmond Youth Project. We first examine whether the model as a whole varies across race, then test key hypotheses about substantive parameters both within and across groups. We examine two substantive issues: (1) the model's ability to explain the influence of family structure on delinquency; and (2) the relative efficacy of differential association versus social control theory.

The data were collected in 1965 as part of the Richmond Youth Project, which sampled a large number of students in 11 junior and senior high schools of Contra Costa County in California (Wilson 1965). These data are particularly well suited to the issues at hand: 1965 marked the publication of Moynihan's report; the population is a large heterogeneous metropolitan area containing substantial numbers of lower-income, inner-city blacks; and the random sample was stratified by race, as well as school, sex, and grade. Our analyses will focus on the

1,588 nonblack males and 1,001 black males.<sup>2</sup> Self-report measures, described in Appendix A, were obtained through questionnaires administered in schools.<sup>3</sup> (For further details of the data collection procedures and characteristics of the sample, see Hirschi 1969.)

Our causal model of delinquency, depicted in Figure 1, consists of a substantive model of the mechanisms generating delinquent behavior and a measurement model of the process by which underlying substantive concepts generate observable measures. The measurement model, indicated by the paths connecting latent variables to

<sup>2</sup> The response rate for nonblacks was 75 percent, for blacks, 68 percent. Hirschi (1969) examined potential bias due to nonresponse, finding that nonresponse was evenly distributed among permission denied by parent, no response by parent, transfers and dropouts, and absentees. Furthermore, while respondents were less likely than nonrespondents to have a police record, this effect did not vary much by race. Therefore, nonresponse should not bias our cross-population results appreciably. (Upon request, covariance matrices of observable variables are available from the authors.)

<sup>3</sup> In using a sample stratified by race, estimating separate models for nonblacks and blacks, and fixing the validity coefficient of self-reported delinquency to be larger for nonblacks than blacks, we are following the recommendations of Hindelang et al. (1981), who argue that, after taking these steps, self-reports of minor forms of delinquency are reasonably reliable and valid for testing theories.

observable indicators, allows us to estimate and control statistically for the biasing effects of measurement error in substantive constructs.<sup>4</sup> Such a model can be crucial for cross-population analyses because it can reveal differential measurement processes across populations, which, if not dealt with, can obscure cross-population comparisons. Therefore, before we proceed to our hypotheses derived from differential association and social control theory, we will examine our measurement models for the two populations.

The substantive component of our model consists of three blocks of variables: four exogenous background variables describing demographic characteristics of individuals, four intervening variables representing the social control and differential association processes, and an outcome variable of self-reported delinquency.<sup>5</sup> We specify the intervening variables—parental supervision (attachment to parents), delinquent friends, attachment to peers, and definitions of delinquency—as linear functions of our background variables: age, socioeconomic status, broken homes, and neighborhood trouble (see Figure 1). In both social control and differential association theories, these effects, representing the influence of social structure on socialization processes, may vary by race.

**HYPOTHESIS 1.** *The effects of background variables, including family structure, on social bonding (attachment and belief) vary by race.*

In addition, differential group organization predicts that attachment to parents and peers and delinquent friends may influence definitions differently by race:

**HYPOTHESIS 2.** *The effects of background variables and parental and peer processes on definitions of delinquency vary by race.*

According to both theories, the total impact of

broken homes and other background variables on delinquency may differ by race. For example, as some previous research has found, broken homes may exert a larger effect on delinquency for blacks than for nonblacks. Whatever the magnitude, however, social control and differential association theories specify intervening mechanisms to account for the total effects. The most significant hypotheses for social control theory are that attachment to parents, attachment to peers, and belief in morality each have a direct effect on delinquency and together should mediate the influence on delinquency of background characteristics such as broken homes, age, SES, and neighborhood trouble. These hypotheses should hold equally for blacks and nonblacks. Furthermore, control theory allows the relative effects of these variables on delinquency to differ by race, reflecting, for example, differential socialization practices across racial groups. The foregoing can be expressed as two hypotheses:

**HYPOTHESIS 3.** *The effects on delinquency of broken homes and the other background variables are mediated by variables representing social bonding.*

**HYPOTHESIS 4.** *Attachment to parents, attachment to peers, and belief all have significant effects on delinquency.*

In contrast, the crucial proposition of differential association theory is that the effects of definitions of delinquency on delinquent behavior should be racially invariant and, for both blacks and nonblacks, should mediate the effects on delinquency of all other variables (see Figure 1). The antecedent variables, including background characteristics and other elements of the social bond, reflect elements of social organization that structure the differential learning of behavior patterns. Consequently, if age, broken homes, or parental supervision have large total effects on delinquency, it is because they represent an important source of learning definitions of delinquency. These propositions translate into two testable hypotheses:

**HYPOTHESIS 5.** *For both blacks and nonblacks, a person's learned ratio of definitions mediates the effects of other antecedent variables in the model, including the effect of broken homes.*

**HYPOTHESIS 6.** *The effect of definitions of the law on delinquency is racially invariant.*

## RESULTS

We estimated the parameters of our measurement and substantive models jointly as a single system using the maximum likelihood estimator of Joreskog's LISREL V program (Joreskog and Sorbom 1984). Our analysis of the measurement

<sup>4</sup> The measurement model of definitions of delinquency conceptualizes Sutherland's concept of a ratio of definitions favorable and unfavorable to delinquency as a unidimensional construct, which generates fallible indicators. Each indicator, measured on a single continuum from highly antidelinquent to highly prodelinquent, is assumed to capture one domain of the ratio of definitions. After controlling statistically for response errors, the common variation across our measures should adequately tap such a construct (see Matsueda 1982 for details).

<sup>5</sup> The causal ordering among our variables within a cross-sectional design follows previous research using these data (Hirschi 1969; Jensen 1972; Matsueda 1982). This ordering is consistent with both differential association theory and social control theory. Some recent evidence on this issue within a longitudinal framework confirms the causal priority of attachment to parents on delinquency (Liska and Reed 1985; Agnew 1985) and definitions of delinquency (belief) (Agnew 1985; Elliott, Huizinga, and Ageton 1985), but see also Minor (1984).

models, discussed in Appendix B, reveals larger measurement errors for blacks than nonblacks. Thus, the failure to correct for attenuation due to unreliability could lead to greater downward biases in regression coefficients among blacks than nonblacks. Overall, the model fits better for nonblacks ( $L^2 = 129.04$  d.f.=71) than blacks ( $L^2 = 216.22$ ; d.f.=71).<sup>6</sup> Both findings are consistent with other similar response models (Bielby et al. 1977).

#### *Estimation of the Model for Nonblacks*

Our discussion of the substantive model will focus on the above six hypotheses. We first highlight the results for nonblacks, then present the findings for blacks in more detail, emphasizing differences across race. Table 1 presents the unstandardized parameter estimates of our baseline model for nonblacks in their reduced, semi-reduced, and structural forms; their standardized counterparts appear in Table 2. These estimates reveal four principal findings. First, the model explains substantial variation in definitions of delinquency ( $R^2 = .66$ ). Friends picked up by the police, attachment to peers, and parental supervision exert substantial direct effects on the learned ratio of definitions (line 10 of Table 2) and also mediate the effects of certain background variables. More precisely, the total effect of neighborhood trouble is mediated by supervision and delinquent friends (compare line 7 with lines 8 and 9). Thus, living in a troubled neighborhood exposes nonblacks to more delinquent definitions by attenuating parental supervision and increasing the number of delinquent companions. Also, the total effect of broken homes on definitions, modest in size but statistically significant, is mediated by parental supervision.

Second, the model also does well in explaining variation in delinquent behavior ( $R^2 = .56$ ): every variable in the model except socioeconomic status has a significant total effect on delinquency. The largest total effect is exerted by delinquent friends, followed by definitions of delinquency, supervision, and neighborhood trouble. Broken homes exert a small but statistically significant total effect.

Consistent with social control theory (Hypothesis 3), our third finding is that the significant

total effects of age, broken homes, and neighborhood trouble are mediated by the joint effects of attachment to parents, delinquent friends, attachment to peers, and moral beliefs (line 15). Thus, being older, from a broken home, and from a troubled neighborhood increases the likelihood of delinquency by attenuating attachments to parents and peers, increasing the number of delinquent friends, and reducing the strength of conventional beliefs.

Nevertheless, our fourth finding, which addresses our crucial test of differential association versus social control theory (Hypothesis 4), supports differential association theory. Both attachment to parents and peers have substantial and statistically significant indirect effects on delinquency through definitions. Moreover, the remaining unmediated direct effects of the attachment variables are not only nonsignificant and small in magnitude, but, from the standpoint of social control theory, implausibly positive in sign (line 15 of Table 1). Thus, as differential association predicts, youths who are closely supervised and develop warm friendships commit fewer delinquent acts because they are exposed to fewer prodelinquent definitions.

In addition, none of the background variables has a significant direct effect on delinquency in the structural form (Hypothesis 5). The number of delinquent friends, however, does have a substantial and statistically significant influence on delinquency.<sup>7</sup> This direct effect is smaller than the effect of definitions, and about as large as the indirect effect of delinquent friends through definitions of delinquency; nevertheless, the result provides some negative evidence for differential association.<sup>8</sup> Although irrelevant to the debate between Hirschi and Sutherland, the finding supports a group process explanation

<sup>6</sup> This holds even though we have a larger sample of nonblacks, and, thus, greater statistical power to detect departure from the hypothesized model. For comparability, the model for blacks includes the same measurement error correlations specified by Matsueda (1982) for the nonblacks—some of which were nonsignificant. A sensitivity analysis, however, revealed that a better-fitting model did not alter the substantive picture in any meaningful way. Thus, it appears that the overall goodness-of-fit statistic is sensitive to trivial departures from uninteresting restrictions.

<sup>7</sup> This is the only finding inconsistent with Matsueda (1982), who found that the influence of delinquent friends on delinquent behavior was mediated by definitions. The discrepancy between our model for nonblacks and Matsueda's (1982) is due to a different method of handling missing values. Here, to insure comparability with the sample of blacks, we used pairwise deletion for nonblacks, while Matsueda (1982) used listwise deletion. We also estimated cross-population models using listwise deletion for both groups, and, while the sample size was reduced by 40 percent, the results were identical for blacks. Thus, missing values do not substantially influence the overall pattern of results.

<sup>8</sup> This direct effect of delinquent friends on delinquent behavior also results in three variables having indirect effects on delinquency through delinquent friends, not definitions of delinquency. The effects are modest in size, however, particularly in comparison to similar indirect effects through definitions. The relative indirect effects through delinquent friends and definitions, respectively, are: .07 and .08 for age, .09 and .19 for neighborhood trouble, and  $-.07$  and  $-.20$  for supervision.

Table 1. Unstandardized Parameter Estimates of the Substantive Model: Nonblack Males ( $N = 1,558$ )

Dependent Variable	Predetermined Variables						Components of Variation					
	AGE	SES	BROKHOME	YOUNGTRO	SUPER	FRPICKUP	ATTACHPE	DEF	R <sup>2</sup>	Residual	Explained	Total
1. SUPER	-.031 (.008)	.004 (.008)	-.154 (.043)	-.107 (.020)					.089	.389	.120	.407
2. FRPICKUP	.157 (.024)	-.022 (.026)	.033 (.132)	.426 (.062)					.121	1.148	.427	1.225
3. FRPICKUP	.134 (.024)	-.019 (.025)	-.082 (.132)	.345 (.063)	-.715 (.122)				.178	1.110	.518	1.225
4. ATTACHPE	.020 (.009)	.015 (.008)	.031 (.041)	-.076 (.020)					.062	.303	.084	.315
5. ATTACHPE	.025 (.010)	.015 (.008)	.055 (.042)	-.059 (.020)	.159 (.041)				.089	.298	.100	.315
6. ATTACHPE	.035 (.010)	.013 (.008)	.050 (.042)	-.035 (.021)	.107 (.040)	-.069 (.016)			.161	.288	.126	.315
7. DEF	.037 (.010)	-.013 (.009)	.111 (.045)	.182 (.025)					.208	.336	.170	.377
8. DEF	.025 (.009)	-.011 (.009)	.052 (.043)	.140 (.023)	-.387 (.053)				.367	.300	.228	.377
9. DEF	.003 (.009)	-.008 (.009)	.065 (.041)	.082 (.021)	-.261 (.045)	.169 (.019)			.614	.235	.295	.377
10. DEF	.012 (.010)	-.004 (.009)	.080 (.041)	.072 (.021)	-.231 (.045)	.149 (.019)	-.282 (.071)		.660	.219	.307	.377
11. DEL	.062 (.018)	.000 (.020)	.227 (.101)	.365 (.047)					.123	.877	.327	.936
12. DEL	.043 (.018)	.002 (.019)	.133 (.101)	.299 (.048)	-.613 (.094)				.188	.844	.405	.936
13. DEL	-.017 (.018)	.011 (.018)	.169 (.096)	.144 (.048)	-.275 (.091)	.450 (.033)			.473	.680	.643	.936
14. DEL	-.011 (.019)	.013 (.018)	.179 (.096)	.137 (.048)	-.254 (.092)	.437 (.035)	-.189 (.151)		.476	.677	.646	.936
15. DEL	-.026 (.021)	.019 (.019)	.083 (.099)	.050 (.052)	.024 (.112)	.257 (.056)	.152 (.180)	1.208 (.292)	.557	.623	.699	.936

Note: Standard errors appear in parentheses.



Table 2. Standardized Parameter Estimates of the Substantive Model: Nonblack Males ( $N = 1,558$ )

Dependent Variable	Predetermined Variables							
	AGE	SES	BROK-HOME	YOUNG-TRO	SUPER	FRPICKUP	ATTACHPE	DEF
1. SUPER	-.123	.017	-.140	-.210				
2. FRPICKUP	.208	-.033	.010	.278				
3. FRPICKUP	.178	-.029	-.025	.225	-.250			
4. ATTACHPE	.106	.092	.037	-.193				
5. ATTACHPE	.132	.088	.066	-.150	.206			
6. ATTACHPE	.180	.080	.059	-.089	.138	-.271		
7. DEF	.160	-.063	.109	.384				
8. DEF	.109	-.056	.051	.296	-.419			
9. DEF	.011	-.040	.064	.173	-.282	.548		
10. DEF	.054	-.022	.078	.152	-.249	.484	-.235	
11. DEL	.108	.000	.090	.311				
12. DEL	.075	.005	.052	.255	-.267			
13. DEL	-.030	.022	.067	.123	-.120	.589		
14. DEL	-.018	.027	.071	.117	-.111	.572	-.063	
15. DEL	-.044	.037	.033	.043	.011	.336	.051	.487

of delinquency (Short and Strodtbeck 1965; Briar and Piliavin 1965).

#### *Estimation of the Model for Blacks*

Parameter estimates of our substantive model for blacks appear in Table 3 in unstandardized form, and Table 4 in standardized form. Our discussion will focus on our cross-population hypotheses. To test these hypotheses, we use likelihood-ratio statistics, which are distributed chi-square in large samples and are obtained by subtracting the pooled likelihood-ratio statistic of our baseline model ( $L^2 = 345.26$ ; d.f. = 142) from that of the model with cross-group constraints. Using the overall test of invariance, we reject the hypothesis that all substantive parameters are the same for blacks and nonblacks ( $L^2 = 427.79$ ; d.f. = 82;  $p < .001$ ) and then proceed to more specific cross-group comparisons. Our first comparison hypothesizes that the determinants of the processes of social bonding and differential association vary by race. For the social control process, the effects of background variables on elements of the social bond (Hypothesis 1) appear invariant across groups ( $L^2 = 20.13$ ; d.f. = 12;  $p > .05$ ). For the differential association process, however, we find that the determinants of definitions of the legal code vary by race ( $L^2 = 18.37$ ; d.f. = 7;  $p < .01$ ). This finding (Hypothesis 2) is due primarily to the larger effects of broken homes, parental supervision, and neighborhood trouble on the process of learning definitions among blacks (compare line 10 in Tables 3 and 4). Thus, from the standpoint of differential association, the neighborhood and family organization of blacks is most telling in the process producing definitions of delinquency.

Turning to the equations predicting delinquent behavior, we first examine the total effects of

our substantive variables and then the causal structure explaining those total effects. Note that delinquent friends have a slightly larger total effect in our model for nonblacks than for blacks. Perhaps the most striking racial difference, however, is in the reduced-form effects of broken homes and neighborhood trouble: the former is three times larger among blacks, while the latter is five times smaller. Thus, consistent with much previous research, broken homes are more influential in producing delinquency among blacks than nonblacks.

Paralleling our findings for nonblacks, we find that, along with delinquent peers, the elements of the social bond—attachments to parents and peers and belief in morality—collectively mediate the influence of our background variables on delinquency (line 15). The indirect effects of age, broken homes, and neighborhood trouble are substantial, while the remaining unmediated effects are either trivial in size (broken homes) or opposite in sign than anticipated (age and neighborhood trouble). Again, this is consistent with social control theory (Hypothesis 3).

We can assess Hypotheses 4 and 5, which test control theory against differential association, by comparing lines 11–15 in Tables 3 and 4. Line 14 reveals that before adding definitions of the legal code into the equation, our model accounts for a substantial amount of variation in delinquency ( $R^2 = .31$ ). Thus, our test of differential association—the extent to which definitions mediate the effects of other variables on delinquency—is a strong one, since substantial total effects must be mediated. As noted above, in the black sample, the reduced-form effect of broken homes on delinquency is substantial (line 11 of Table 4), as is the semi-reduced form (line 14). Before adding definitions into the equation, then, broken

Table 3. Unstandardized Parameter Estimates of the Substantive Model: Black Males ( $N = 948$ )

Dependent Variable	Predetermined Variables						Components of Variation					
	AGE	SES	BROKHOME	YOUNGTRO	SUPER	FRPICKUP	ATTACHPE	DEF	R <sup>2</sup>	Residual	Explained	Total
1. SUPER	-.015 (.008)	.010 (.014)	-.178 (.058)	-.057 (.025)					.066	.332	.084	.344
2. FRPICKUP	.112 (.033)	-.032 (.056)	.347 (.221)	.190 (.096)					.063	1.108	.288	1.145
3. FRPICKUP	.097 (.032)	-.022 (.055)	.166 (.223)	.132 (.096)	-1.021 (.200)				.151	1.055	.445	1.145
4. ATTACHPE	.001 (.011)	.029 (.017)	.041 (.065)	-.050 (.029)					.035	.324	.063	.330
5. ATTACHPE	.004 (.011)	.027 (.017)	.072 (.068)	-.040 (.029)	.175 (.063)				.066	.318	.008	.330
6. ATTACHPE	.012 (.011)	.025 (.017)	.087 (.068)	-.029 (.029)	.086 (.065)	-.088 (.026)			.145	.305	.126	.330
7. DEF	.073 (.024)	.037 (.030)	.531 (.119)	.243 (.052)					.326	.437	.303	.532
8. DEF	.063 (.023)	.044 (.029)	.409 (.114)	.204 (.050)	-.688 (.123)				.509	.373	.379	.532
9. DEF	.049 (.024)	.047 (.029)	.384 (.112)	.184 (.049)	-.534 (.122)	.150 (.038)			.598	.338	.411	.532
10. DEF	.053 (.024)	.056 (.029)	.414 (.114)	.174 (.049)	-.504 (.121)	.119 (.041)	-.349 (.136)		.638	.319	.425	.532
11. DEL	.003 (.024)	.060 (.041)	.697 (.164)	.075 (.071)					.096	.794	.258	.835
12. DEL	-.007 (.024)	.067 (.041)	.576 (.166)	.037 (.071)	-.681 (.146)				.170	.761	.345	.835
13. DEL	-.034 (.024)	.073 (.041)	.530 (.165)	.000 (.071)	-.394 (.157)	.281 (.056)			.295	.701	.454	.835
14. DEL	-.030 (.024)	.082 (.041)	.560 (.167)	-.011 (.071)	-.364 (.156)	.250 (.060)	-.353 (.198)		.312	.693	.467	.835
15. DEL	-.082 (.041)	.026 (.051)	.149 (.245)	-.183 (.106)	.136 (.253)	.131 (.077)	-.007 (.255)	.992 (.371)	.456	.616	.565	.835

Note: Standard errors appear in parentheses.

Table 4 Standardized Parameter Estimates of the Substantive Model: Black Males ( $N = 948$ )

Dependent Variable	Predetermined Variables							
	AGE	SES	BROK-HOME	YOUNG-TRO	SUPER	FRPICKUP	ATTACHPE	DEF
1. SUPER	-.070	.039	-.180	-.135				
2. FRPICKUP	.162	-.039	.106	.136				
3. FRPICKUP	.141	-.027	.051	.094	-.306			
4. ATTACHPE	.006	.121	.043	-.125				
5. ATTACHPE	.019	.114	.076	-.100	.183			
6. ATTACHPE	.062	.106	.092	-.071	.089	-.306		
7. DEF	.228	.097	.348	.372				
8. DEF	.197	.115	.268	.312	-.444			
9. DEF	.151	.123	.252	.281	-.345	.323		
10. DEF	.165	.146	.272	.266	-.325	.257	-.216	
11. DEL	.006	.101	.291	.074				
12. DEL	-.014	.112	.241	.036	-.280			
13. DEL	-.068	.122	.221	.000	-.162	.385		
14. DEL	-.059	.137	.234	-.010	-.150	.342	-.139	
15. DEL	-.164	.044	.062	-.178	.056	.180	-.003	.632

homes have a large and significant effect on delinquency. After adding definitions, however, that effect becomes trivial in size and statistically indistinguishable from zero (line 15 of Table 3). As differential association predicts, broken homes influence delinquency by impeding the transmission of antidelinquent definitions and increasing the transmission of prodelinquent patterns. Similarly, in accord with control theory, attachment to parents (supervision) has a large total effect on delinquency that works partly indirectly through delinquent friends and partly directly before adding our definitions variable. But the structural form (line 15) reveals that, after adding definitions into the equation, the effect of supervision becomes nonsignificant, and, from the standpoint of control theory, implausibly positive. Again, this is consistent with differential association theory: supervision influences delinquency by influencing the ratio of learned definitions of delinquency. Furthermore, delinquent friends exert a large and significant effect on delinquency before adding definitions, but a comparatively small and nonsignificant effect in the presence of our definitions construct (compare lines 14 and 15). Thus, in contrast to our findings for nonblacks, delinquent friends influence delinquency by presenting definitions of the legal code; this finding supports differential association theory over group process theories.<sup>9</sup> While

the total effect of attachment to peers is small and statistically nonsignificant, the indirect effect through definitions is significant, rendering the direct effect on delinquency virtually nonexistent. Differential association is again supported over control theory.

Finally, we tested Hypothesis 6, derived from differential association theory, which postulates that the effect of definitions on delinquency is invariant across race. That test confirmed the hypothesis: the point estimates are indistinguishable from one another at conventional levels of significance ( $L^2 = .17$ ;  $d.f. = 1$ ;  $p > .50$ ). Thus, differential association theory again receives strong support.

## DISCUSSION

For both black and nonblack samples, our models support differential association theory over social control theory. Contrary to Hirschi's (1969) postulate that each element of the social bond shows a unique and substantial effect on delinquency, we find that the effects of attachment to parents and peers operate indirectly through the process of learning an excess of definitions favorable to delinquency. This finding is consistent with differential association theory, as are the findings that across racial groups, the effect of definitions on delinquency is invariant, and within groups, definitions mediate the influence on delinquency of our other explanatory variables.

Of more interest are the differences between our models for blacks and nonblacks. The most

<sup>9</sup> The nonsignificance of this parameter estimate for blacks could be due to type II error, given the smaller size of the black sample. To investigate this, we conducted a power analysis, following the recommendation of Matsueda and Bielby (1986). We found that the model for blacks had ample statistical power (.95) to detect a metric coefficient the size of the estimate for nonblacks. But, although we cannot detect, with reasonable power, a standardized coefficient of .20

(power = .50), we can detect a standardized coefficient of .23 (power = .65). Thus, we have sufficient protection against type II error, assuming a nontrivial (larger than .20) effect of delinquent friends on delinquency in our black population.

striking difference is that the total effect of broken homes on delinquency is much larger for blacks than nonblacks. Yet in both racial groups nonintact homes influence delinquency through a similar process—by attenuating parental supervision, which in turn increases delinquent companions, prodelinquent definitions, and, ultimately, delinquent behavior. But to a much greater extent, broken homes *directly* foster an excess of definitions favorable to delinquency, which then increases delinquent behavior. This effect, being much larger among blacks, accounts for the greater total effect of broken homes on delinquency among blacks.

A second racial difference is the total effect of neighborhood trouble on delinquency, which is much larger in the model for nonblacks. Among nonblacks, that effect works partly through delinquent friends, but largely through definitions of delinquency; among blacks, a large indirect effect operates solely through definitions. We also examined an interaction hypothesis between neighborhood trouble and broken homes: Do broken homes influence delinquency only in the context of a trouble-ridden, high delinquency neighborhood? Entering a product variable representing the interaction effect, we found evidence of a conditional effect among blacks but not nonblacks. Blacks from broken homes who also live in troubled neighborhoods are more likely than those residing in trouble-free neighborhoods to associate with delinquents, learn an excess of definitions favorable to delinquency, and, consequently, violate the law. We were unable to locate such an interaction in the nonblack model, perhaps due to multicollinearity among main and interaction effects.<sup>10</sup>

We should note that, following a long history of research on family structure and delinquency, we have used a single dichotomous variable to distinguish intact from nonintact homes. Recently, some have argued that the impact of family structure on delinquency may vary depending on the nature of that structure, such as whether a step-parent is present or whether the mother or father is absent (Rankin 1983; Johnson 1986; Wells and Rankin 1986). We were unable to examine the joint relationships among different forms of family structure, our intervening variables, race, and delinquency because the small number of cases falling into

each category of family status led to multicollinearity and unstable estimates. Other research suggests that the etiology of the break, such as death, divorce, or desertion, can influence subsequent behavior (McLanahan 1985). Furthermore, many argue that the pertinent variable is marital and familial discord, which could have an adverse effect on intimacy, supervision, and the transmission of antidelinquent behavior patterns, and which could also cause a marital breakup. Since marital severance is also likely to cause discord, cross-sectional research designs are likely to confound the causes and consequences of family disruption. Longitudinal designs are needed to disentangle the reciprocal effects of family process, family structure, and delinquent behavior. Based on the results of our models, we expect that the key intervening mechanism explaining the effects on delinquency of such family processes is the learning of delinquent and antidelinquent definitions.

But the link between definitions of delinquency and social structure may be more complex than implied thus far and may suggest another empirically testable divergence between the theories of differential association and social control. More precisely, social control theory, based on a consensus model of social order, denies the efficacy of competing subcultural norms and assumes that only conventional norms and definitions of morality influence behavior. In contrast, differential association theory, based on a group conflict model of social organization, specifies that subcultural groups may differ on two dimensions of definitions of delinquency—the dimension of the weight of the definition, and, more importantly, the dimension of the meaning or content of the definition (Matsueda 1982). The latter implies that groups located at different junctures in the social structure may communicate and behave according to very different definitions of unlawful behavior. In particular, the content of definitions of delinquency may vary by race, neighborhood, and social class.<sup>11</sup> To explore this issue, researchers must first use in-depth interviews to induce the content of such definitions for distinct communication groups and then develop empirical measures to tap such definitions. Structural equation methods within

<sup>10</sup> Large bivariate correlations between product variables and their constituents suggest the problem of multicollinearity in disentangling interaction effects from main effects in both samples. Thus, we treat these results with caution. We also failed to unearth interaction effects among SES, broken homes, and neighborhood trouble, which was expected, given the null effects of SES on the endogenous variables.

<sup>11</sup> We attempted to explore this inductively using the Richmond data. That is, with our confirmatory factor models of definitions, we examined the possibility that some indicators that are valid for nonblacks are invalid for blacks, and vice versa. By and large, we did not find such differences in validity across race; what was a strong indicator in one population was generally strong for the other. This is not surprising, of course, since the measures are global, and designed to apply across general populations.

the LISREL framework exist for making cross-population comparisons when indicators for concepts differ across groups (Allison 1985).

Viewed in broader perspective, our results raise larger questions concerning the role of social structure on race, cultural norms, and delinquency. That is, given that delinquency is largely determined by the learning of definitions of the legal code, what are the wider structural determinants of that learning process? Our ability to explain remarkably large amounts of variation in definitions with a small number of variables suggests that such a learning process is tightly structured. When examining a single cross-section of individuals, we find that the learning process is structured by elements of social organization such as age, neighborhoods, families, and peers. Moreover, the differential impact of these structures accounts, in large part, for racial disparities in delinquent behavior.

From both a scientific and policy standpoint, a more significant issue may be the historical emergence of social and economic structures that give rise to distinct racial patterns of social organization. Thus, the racial cleavages in normative definitions of delinquent behaviors may derive from a history of restricted opportunities, a sense of resignation, and, ultimately, new ways of adapting to a bleak situation (Cloward and Ohlin 1960). For example, William Julius Wilson (1985) argues that increasing social dislocations among the urban underclass were a culmination of a number of demographic, economic, and cultural changes. Specifically, the increasing disparity in crime across race is a result of historic not contemporary discrimination, the unabated migration of Southern blacks to the centers of Northern cities, the drop in age structure among inner-city blacks, and a general economic shift from a manufacturing to a service economy. In turn, these broad historical trends have led to different patterns of social organization among the urban underclass, which influence rates of delinquency. For example, we have shown that delinquency is in part spawned by broken homes, unsupervised family life, ineffective neighborhood organization, and, ultimately, differential association. If this historical explanation is correct, and the critical learning process is indeed interwoven in the fabric of such historical trends, it should be no surprise that simplistic policies of rehabilitation and deterrence have failed to stem the tide of rising rates of delinquency. Sweeping social and economic reforms may be necessary to reverse the strong currents of law violation (Wilson 1985).

In the absence of a substantial body of empirical research verifying these propositions,

however, such theorizing is speculative. Nevertheless, such speculation is consistent with our principal findings that the influence of broken homes on delinquency is greater among blacks; that this influence is explained by the process of learning definitions of delinquency; and that, for both blacks and nonblacks, differential association theory is supported over social control theory.

## APPENDIX A

### *Key to Variable Labels*

AGE: Age of respondent. 0 = 12 years or younger, 1 = 13 years, 2 = 14 years, 3 = 15 years, 4 = 16 years, 5 = 17 years, 6 = 18 years, 7 = 19 years, 8 = 20 years or older.

BELIKFR: "Would you like to be the kind of person your best friends are?" 0 = not at all, 1 = in a few ways, 2 = in most ways.

BROKHOME: A dummy variable coded as one if either the mother or the father did not live with the respondent.

DEL: An index of delinquency committed during the last year containing the following six items:

BATTERY: "Not counting fights you may have had with a brother or sister, have you ever beaten up on anyone or hurt anyone on purpose?"

CARTHEFT: "Have you ever taken a car for a ride without the owner's permission?"

THEFT2: "Have you ever taken little things (worth less than \$2) that did not belong to you?"

THEFT250: "Have you ever taken things of some value (between \$2 and \$50) that did not belong to you?"

THEFT50: "Have you ever taken things of larger value (over \$50) that did not belong to you?"

VANDALSM: "Have you ever banged up something that did not belong to you on purpose?"

DELHURT: "Most things that people call 'delinquency' don't really hurt anyone." Strongly disagree, disagree, undecided, agree, strongly agree.

EVNBREAK: "Policemen try to give all kids an even break." Strongly agree, agree, undecided, disagree, strongly disagree.

FRPICKUP: "Have any of your close friends ever been picked up by the police?" 0 = no or don't know, 1 = one friend has, 2 = two friends have, 3 = three friends have, 4 = four or more friends have.

GETAHEAD: "To get ahead, you have to do some things which are not right." Strongly disagree, disagree, undecided, agree, strongly agree.

OKLAW: "It is alright to get around the law if you can get away with it." Strongly disagree, disagree, undecided, agree, strongly agree.

PARWITH: A composite asked regarding each parent: "Do your parents know who you are with when you are away from home?" 0 = never-never, 0.5 = sometimes-never, 1.0 = sometimes-sometimes, 1.5 = usually-sometimes, 2.0 = usually-usually.

PARWHERE: Same as above but with the question: "Do you parents know where you are when you are away from home?"

RESPECTFR: "Do you respect your best friend's

opinions about the important things in life?" 0 = not at all, 1 = a little, 2 = pretty much, 3 = completely.

RSPECTPO: "I have a lot of respect for the Richmond police." Strongly agree, agree, undecided, disagree, strongly disagree.

SES: Father's occupation measured on the Duncan Scale; if there is no father living in the home, mother's occupation is used. For the few cases in which father's occupation had a missing value, and father's education was reported, values of father's occupation were predicted by regressing occupation on education.

SUCKERS: "Suckers deserve to be taken advantage of." Strongly disagree, disagree, undecided, agree, strongly agree.

TROUBLE: "I can't seem to stay out of trouble no matter how hard I try." Strongly disagree, disagree, undecided, agree, strongly agree.

YOUNGTRO: [In my neighborhood] "Young people are always getting into trouble." Strongly disagree, disagree, undecided, agree, strongly agree.

## APPENDIX B

### *Analysis of the Measurement Models*

Two issues concerning our measurement models require attention: (1) whether, as previous studies suggest, blacks respond with greater random variation; and (2) whether the metrics of latent variables appear equivalent across groups, allowing straightforward cross-population comparisons. Parameter estimates of our measurement models appear in Table A1. Column 2 reveals that, with the exception of age, the means of our observable variables all differ significantly across race. On average, nonblacks have higher socioeconomic status, fewer broken homes, less troubled neighborhoods, more parental supervision, fewer delinquent friends, more attachment to peers, a lower ratio of definitions favorable and unfavorable to delinquency, and fewer self-reported delinquent acts. Our analyses leave the observable means unconstrained.

Column 3 reveals that the observed indicators show more variation among blacks than nonblacks. This is due to uniformly larger random response errors among blacks, as indicated in column 4, a finding that is consistent with previous research. A formal test of invariant error variances revealed that invariance is rejected for measures of the two attachment constructs ( $L^2 = 41.98$ ; d.f. = 4;  $p < .001$ ), and also rejected for our measures of definitions ( $L^2 = 77.10$ ; d.f. = 7;  $p < .001$ ). The validity coefficients indicate a similar ordering of accurate indicators for both races: RSPECTFR is a better indicator of attachment to peers than BELIKFR; and OKLAW, RSPECTPO, and TROUBLE are more accurate measures of definitions. Following Matsueda (1982), we fixed the error variance of AGE to

reflect a validity of .95, and fixed the error variances of SES, BROKHOM, YOUNGTRO, FRPICKUP, and DEL to reflect a validity of .80 for nonblacks and .70 for blacks. These values follow previous research, which finds larger measurement error variances among blacks than nonblacks for measures of socioeconomic status (Bielby et al. 1977) and delinquency (Hindelang et al. 1981). A sensitivity analysis varying the validity coefficients from .95 to .60 for nonblacks, and from .85 to .60 for blacks, did not appreciably change our substantive parameter estimates.

Column 5 presents the metric (unstandardized) slopes of our measures. We found that the metric slopes for indicators of parental supervision and attachment to peers are statistically indistinguishable across groups (lambda invariant) ( $L^2 = 5.06$ ; d.f. = 2;  $p > .05$ ), a finding that allows us to make straightforward cross-population comparisons of metric coefficients involving supervision and attachment. Because we have to normalize our indicators (here, by fixing the metric slope of one indicator of each construct to unity), only the ratios of metric slopes are identified. Consequently, lambda invariance literally means that the *ratios* of metric slopes of a given construct are invariant across groups (Bielby 1986). We also found the two metric slopes of supervision statistically indistinguishable within race (tau-equivalent) ( $L^2 = 5.43$ ; d.f. = 2;  $p > .05$ ), but rejected tau-equivalence for indicators of attachment to peers ( $L^2 = 7.38$ ; d.f. = 2;  $p < .05$ ).

For indicators of DEF, however, metric slopes appear dissimilar across groups ( $L^2 = 19.42$ ; d.f. = 6;  $p < .005$ ) and across indicators ( $L^2 = 81.96$ ; d.f. = 12;  $p < .001$ ). Relative to the reference indicator (TROUBLE), most of the other indicators in the black population have flatter slopes. Thus, relative to TROUBLE, blacks scoring high on the other indicators tend to understate their true definitions of delinquency, and those scoring low tend to overstate. By and large, the opposite holds for nonblacks: relative to TROUBLE, most of the other indicators have *steeper* slopes. This suggests that blacks use slightly different metrics than nonblacks in interpreting the Likert-scale indicators of definitions. In turn, this implies that cross-population comparisons of our unstandardized regression coefficients among substantive constructs could vary depending on which indicator we choose to normalize on (Bielby 1986; Williams and Thomson 1986). Therefore, we performed a sensitivity analysis, varying the reference indicator for the latent construct underlying our measures of definitions of delinquency. That analysis revealed no substantial differences in black-nonblack comparisons of regression coefficients, suggesting that our results are not sensitive to the choice of reference indicator. Given this, it seems reasonable to assume that we can make meaningful cross-population comparisons of metric coefficients.

# RACE, FAMILY STRUCTURE, AND DELINQUENCY

Table A1. Parameter Estimates of the Measurement Models: Black (N = 948) and Nonblack Males (N = 1,558)

Latent Variable (1)	Observable	Observed Mean <sup>a</sup> (2)		Observed Variance (3)		Error Variance (4)		Metric Slope (5)		Validity Coefficient (6)	
		Blacks	Nonblacks	Blacks	Nonblacks	Blacks	Nonblacks	Blacks	Nonblacks	Blacks	Nonblacks
1. AGE	AGE	2.79	2.86	3.108	2.977	.311f	.298f	1.000f	1.000f	.949f	.949f
2. SES	SES	2.36	3.96	3.994	5.572	2.037f	2.006f	1.000f	1.000f	.700f	.800f
3. BROKHOME	BROKHOME	0.55	0.31	.248	.215	.126f	.077f	1.000f	1.000f	.700f	.800f
4. YOUNGTRO	YOUNGTRO	2.91	2.43	1.355	.996	.691f	.359f	1.000f	1.000f	.700f	.800f
5. SUPER	PARWITH	2.29	2.56	.351	.308	.233	.142	1.000f	1.000f	.580	.734
6. SUPER	PARWHERE	2.42	2.61	.321	.291	.113	.133	1.329	.972	.806	.736
7. FRPICKUP	FRPICKUP	1.48	1.09	2.672	2.344	1.363f	.844f	1.000f	1.000f	.700f	.800f
8. ATTACHPE	BELIKFR	1.95	2.14	.482	.395	.373	.296	1.000f	1.000f	.475	.500
9. ATTACHPE	REPECTFR	2.51	2.70	.724	.529	.450	.354	1.588	1.331	.615	.574
8. DEF	EVNBREAK	2.84	2.59	1.609	1.506	1.556	1.355	.434	1.030	.182	.318
9. DEF	DELHURT	3.07	2.77	1.237	1.174	1.182	1.034	.441	.994	.212	.343
10. DEF	OKLAW	2.55	2.12	1.450	1.166	1.155	.860	1.020	1.464	.452	.512
11. DEF	RSPECTPO	2.58	2.41	1.362	1.243	1.183	.930	.791	1.475	.362	.502
12. DEF	GETAHEAD	2.86	2.43	1.568	1.255	1.447	1.053	.653	1.187	.277	.401
13. DEF	TROUBLE	2.49	2.22	1.360	1.008	1.077	.865	1.000f	1.000f	.456	.377
14. DEF	SUCKERS	2.86	2.53	1.457	1.202	1.332	1.020	.662	1.126	.293	.389
15. DEL	DEL	0.89	0.79	1.432	1.370	.726f	.493f	1.000f	1.000f	.700f	.800f

Notes: All parameter estimates significant at the .001 level. f = fixed coefficient.

<sup>a</sup> The difference between means for blacks and nonblacks are significant at the .001 level for all variables except AGE.

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*This paper examines the determinants of initiation of coitus in early adolescence. Using a panel design on the population of whole schools we tested determinants in three domains: motivation, social controls, and attractiveness. We obtained measures of variables from respondents, their identified friends, parents, and interviewers. We interpret the results within the framework of other findings from a hormone analysis of a partially overlapping sample. White males' initiation of coitus in early adolescence is dominated by motivational hormone effects and social attractiveness, with no observed effects of social controls. White females' initiation of coitus is dominated by the effects of social controls. We observed no effect of attractiveness, no hormone effects, and no effects of sexual motivation. Black females' initiation of coitus is dominated by their level of pubertal development (an attractiveness variable), with no observed effects of social controls.*

## INTRODUCTION

Adolescent sexual behavior has long been treated within the framework of the sociology of deviant behavior (Reiss 1970). Most sociologists have conceptualized early adolescent sexual behavior as a failure of age-graded social controls, with the primary focus on the effects of normative control by parents and peers (Hogan and Kitegawa 1985; Thornton and Camburn 1983).

Sexual motivation is assumed to emerge during early adolescence. We assume that, in the case of two adolescents similarly socially constrained, the one with higher motivation is more likely to begin sexual behavior. Our models also include another dimension that is usually ignored: to have coitus, one must have a partner. Since in this "deviant behavior" the partner is not usually a victim but a willing participant, the ability to attract a partner must be considered in the model.

Having conceptualized adolescent coitus as the failure of age-graded controls, we propose three dimensions that determine the probability of escaping the age-graded controls: motivation, attractiveness, and social controls. Figure 1

diagrams the conceptual model. On the right of the diagram is the dependent variable, transition to first intercourse. The next column to the left gives the basic dimensions of the model, and the next to the left gives the components of the basic dimensions. The column on the far left gives the measures of each component.

## Motivation

Motivation has two generic origins: biology as a source of libido, and social evaluation. The traditional model of adolescent sexuality assigns a primary role to adolescent hormonal increases associated with puberty. Social processes inhibit overt sexual expression until a socially appropriate age is reached, and then see that it is expressed in socially appropriate forms with partners in socially sanctioned relationships. Sexual behavior is, therefore, a special case of an age-graded behavior, whose primary base is biological. Udry et al. (1985, 1986) have shown that androgens (male hormones) are related to sexual interest and behavior in male and female adolescents.

Social scientists have noted that the data supporting the hormonal basis of adolescent sexual interest are unconvincing. They have proposed an alternate model that relegates hormones to the role of causing pubertal development, which, in turn, serves as a social signal to society and the individual that the age-graded entry into sexual behavior is appropriate and desirable (Gagnon and Simon 1973). Billy and Udry (unpublished) and Smith et al. (1985) have shown that pubertal development is an important contributor to sexual behavior in adolescence.

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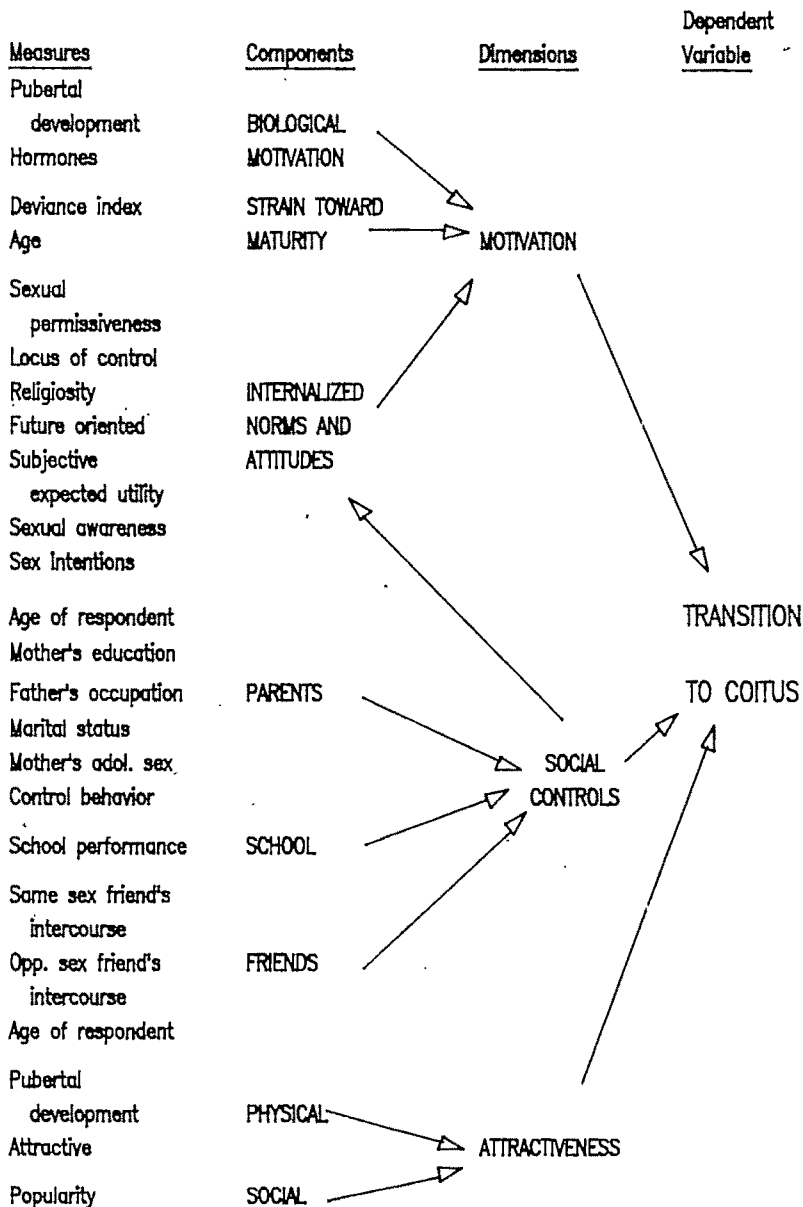


Fig. 1. Conceptual Model for Transition to Coitus

Strain toward maturity, or "proneness to engage in problem behavior," is defined by Jessor and Jessor (1975) as "behaviors that depart from the regulatory norms defining what is appropriate for that age or stage in life." They specifically include coitus as one of these behaviors, and use an index of other such behaviors to predict coitus transition proneness. They report that those virgins who show other age-graded norm violations are more likely to make coital transitions in the near future. In our framework, the index becomes an additional measure of motivation for coitus, not as a

specific sexual motivation, but as motivation to get on with behaviors reserved for older ages.

A final component of motivation is derived from the normative structure. The reward/punishment aspect of the normative structure in which the individual is embedded is a source of socially generated motivation. Adolescents internalize the behavior standards of parents, friends, and school, and form attitudes of greater or lesser sexual permissiveness, religiousness, future orientation, and the like. In this way, social controls affect motivation through attitudes. Many studies have examined attitudes as related

to initiation of coitus (Cvetkovich and Grote 1976; Jessor and Jessor 1975).

### *Attractiveness*

Physical attractiveness has two components. One component is having the secondary sexual characteristics of a mature member of one's sex. In early adolescence, individuals at the same age differ greatly in their pubertal development and, therefore, in the degree to which they will be sexually attractive to members of the opposite sex. The second component is handsomeness or prettiness, i.e., the extent to which, independent of pubertal development, the individual's facial and bodily appearance conforms to standards of beauty for each sex. We hypothesize that opportunities for sex will differ according to physical attractiveness, and can measure this by measuring pubertal development and rating attractive features.

Establishing heterosexual relationships, which create the circumstances for consensual coitus, requires learning intricate social patterns. Among adolescents, we can conceptualize the skills associated with learning these patterns as the ability to make friends, or social attractiveness, and measure it by the popularity of the individual, or the number of times selected as a friend by others. Newcomer et al. (1983) reported that popularity with opposite-sex friends (but not same-sex friends) is associated with being coitally experienced for white boys but not for white girls or for blacks. However, Newcomer et al. used cross-sectional data and were not able to determine whether popularity caused sexual activity or the reverse.

### *Social Controls*

Not all adolescents encounter the same normative environment; we might, therefore, expect each adolescent to experience the social controls differently. If so, it should be possible to predict which adolescents will experience the social controls most strongly, and, therefore, be least likely to engage in early sex. Social controls may be conceptualized into two components: (1) restrictions and opportunities presented by the social environment; and (2) internalized controls. Parents and friends may inhibit or facilitate the sexual behavior of adolescents by providing or restricting opportunities, rewarding or punishing the adolescents, or by inculcating norms that are internalized by the adolescents.

*Parents.* Several researchers believe socioeconomic status (SES) of parents to be related to parental ability to control their adolescents (Hogan and Kitagawa 1985; Kantner and Zelnik 1972; Thornton and Camburn 1983; Udry et al. 1975). Studies (Bowerman et al. 1963; Hogan &

Kitagawa 1985; Kantner & Zelnik 1972; Moore & Furstenburg 1984; Newcomer & Udry 1987 [using the present data set]; Thornton & Camburn 1983) have also found marital status of the parents to be related to coital initiation rates, with low rates related to two-parent homes. Further, the mother's own early sexual behavior may influence her child's behavior, either through her attitudes or through other routes. This issue has been examined for the mother's premarital pregnancy by Thornton and Camburn (1983) and for her adolescent sexual behavior by Newcomer and Udry (1985b). Finally, several studies have found that parental attitudes, parent-child communication, and parental control behavior as reported by the adolescent are related to initiation of coitus (Hogan and Kitagawa 1985; Jessor and Jessor 1975; Newcomer and Udry 1985).

*Friends.* It has been axiomatic that friends influence the sexual behavior of adolescents. Most studies have shown that adolescents perceive their friends to have similar sexual behavior to their own (Carns 1973; DeLameter and MacCorquodale 1979; Jackson and Potkay 1973; Mirande 1968; Shah and Zelnik 1981; Spanier 1975; Teeven 1972). This has been interpreted routinely to mean that the friends influence the respondents. However, when friends' reports of their own sexual behavior are used, and proper models are constructed to distinguish influence, acquisition, and deselection of friends, Billy and Udry (1985) found that only white females, not white males or black females, are differentially influenced by their friends' sexual behavior.

## DATA AND METHODS

### *Study Design and Sample*

Nearly all previous studies of the determinants of adolescent sexual involvement share two characteristics. First, most are cross-sectional. Characteristics of the respondents at interviews are used to explain behavior that occurred before the interview. Only the panel study of Jessor and Jessor (1975) examines characteristics measured at one time, predicting transition to intercourse at a later time. Second, the adolescent respondent is in nearly every case the only source of information used to assess the impact of parents, friends, and other potential influences on sexual behavior. Researchers usually sidestep the problem of the adolescent inaccurately perceiving the environment by saying that it does not make any difference what that environment is like. It only makes a difference how the adolescent perceives it. This social interactionist approach begs the whole question of social influence, since it never escapes the adolescent's own perceptual framework.

Our study remedies both these problems, but at the expense of the general representativeness of the sample. First, it is a panel study in which several variables in each of the domains described above are measured at a period prior to the time when the sexual behavior of interest (first intercourse) occurred. At the time of the first interview, some respondents will already have had coitus, perhaps even years before. Our design cannot possibly identify the antecedents of coitus for this subgroup, since the theoretically relevant time has already passed. We have demonstrated in preliminary analysis that, when variables measured at the first interview (time 1) are used to "predict" coitus occurring prior to that time, entirely different "findings" emerge from those based on using time 1 variables to predict subsequent coitus for time 1 virgins. We reject the retrospective "prediction" model and consider the determinants of coitus for time 1 nonvirgins to be unknowable within the confines of our study design. We are limited to answering the question, What are the determinants of transition to coitus for those who "survived" to first interview as virgins?<sup>1</sup>

Second, we use data from sources other than the adolescent to examine variables that might be contaminated by the adolescent's perceptual processes, or about which the adolescent has only imperfect information. Parental influence is measured using parents' reports. Friendship influence is measured using friends' reports. Attractiveness of the adolescent is measured using interviewers' reports. Popularity is measured using friends' nominations.

The sample and study design were dominated by the need to identify and interview the friends of respondents. This led us to define as our target sample all pupils enrolled in selected schools. The schools were selected opportunistically for the availability of a list of pupils for the investigators. The population finally identified was all pupils in grades 7, 8, and 9 at selected public schools in a medium-sized southern city. Interviewers visited the homes of pupils early in 1980, explained the study to a parent or guardian, and requested the parent's permission for the adolescent to participate in the study. Once signed permission was obtained from the parent, the adolescent was requested to participate. Of the eligible population, losses due to refusal were 27 percent, about equally

divided between parental and adolescent refusals. This netted an initial sample of 1,405 respondents. Respondents filled out a self-administered questionnaire with the interviewer in the home. Respondents ranged from 11 to 17 years old, with a mean age of 13.6, and represented a wide variety of socioeconomic levels.

Approximately two years later, an essentially identical follow-up questionnaire was administered to the original sample. Of the 1,405 respondents from the first round, 1,153 completed questionnaires in round 2, an 82 percent follow-up rate. Almost all between-round attrition was due to out-migration. At the time of the second interview, adolescents were in grades 9, 10, and 11, ranging in age from 13 to 19 with a mean age of 15.4. During both interviews the mother of the respondent also completed a shorter self-administered questionnaire. About two-thirds of the mothers of adolescent respondents completed usable questionnaires. The interviewer also completed a short form during each round, rating the adolescent on attractiveness and social and physical maturity.

### *Measurement of the Variables*

All variables used to predict the transition to intercourse between rounds 1 and 2 are measured at time 1. A description of each measure is presented in the appendix. Table 1 presents the means and standard deviations of each variable by race-sex subgroup.

### *Statistical Models*

With so many variables at several different levels, a fully specified causal model is virtually impossible; many simplifying assumptions are required to test the conceptual model. The variables are entered sequentially into a logistic regression (because of the dichotomous dependent variable) according to a specified causal priority. The first criterion for causal order is timing of the event. The second criterion is theory. When timing and theory cannot resolve the order, we decide by fiat.

Nothing in the model can affect pubertal development except age. If age is entered first, it will contain a component related to pubertal development. If pubertal development is entered first, then the remaining explanatory power of age will be that part not related to pubertal development. This order was selected. Attractiveness was entered third, because it might be affected by age and pubertal development.

Parental variables of education, occupation, household structure, and mother's adolescent

<sup>1</sup> Some readers may identify our research problem as calling for a selection bias correction technique. All "selection bias correction" models applied to our data assume that it doesn't make any difference whether the predictors are measured before or after coitus. For a demonstration of how selection bias correction may exacerbate the problem it purports to correct, see Stolzenberg and Relles (1987).

Table 1. Means and Standard Deviations of Variables Used to Predict Transition to Intercourse Between Rounds 1 and 2, by Race-Sex Subgroup

Variables	White X	Females s.d.	White X	Males s.d.	Black X	Females s.d.
<i>Adolescent Variables</i>						
Pubertal development	-.06	.92	-.19	.88	-.10	.91
Age	13.94	.94	13.87	.99	14.14	1.18
Attractiveness	4.57	1.09	4.53	1.07	4.51	1.00
Parental household structure	.71	.45	.73	.45	.42	.50
Same-sex friend's intercourse	.08	.28	.21	.40	.30	.46
Opposite-sex friend's intercourse	.36	.48	.10	.30	.84	.37
Popularity with opposite sex	1.81	2.13	1.42	2.35	1.80	2.28
School performance	2.51	.56	2.39	.60	2.02	.59
Deviant behavior	2.99	1.16	2.90	1.20	1.93	1.21
Noncoital sexual experience	-.17	.83	-.44	.73	-.40	.52
Locus of control	4.54	1.21	4.57	1.24	4.54	1.68
Future orientation	3.43	1.35	3.77	1.23	3.35	1.41
General sexual permissiveness	4.25	1.49	4.76	1.40	4.23	1.45
Premarital sexual permissiveness	14.26	4.76	15.70	4.97	16.81	5.19
Thinking about sex	3.35	1.40	4.62	1.29	2.50	1.49
"Turn-on" index	4.62	2.49	5.53	2.00	3.60	2.98
Subjective expected utility of sex	-34.31	30.13	-6.33	25.49	-34.66	32.68
Intentions to have sex	5.40	2.32	8.09	2.39	6.10	2.49
Transition to intercourse	.26	.44	.31	.47	.38	.49
Popularity with same sex	2.10	1.66	1.86	1.64	2.23	1.66
Self-satisfaction	9.75	1.62	9.99	1.60	10.07	1.71
Importance of religion	2.63	.80	2.43	.86	3.41	.72
N		280		264		86
<i>Parental Variables</i>						
Mother's education	16.56	3.41	16.65	3.32		
Father's occupation	4.19	.94	4.16	.91		
Maternal control	3.34	1.60	3.25	1.49		
Mother's sexual experience	1.73	1.41	1.49	1.25		
Mother's premarital permissiveness	3.44	1.36	3.49	1.47		
Mother's religiousness	2.99	.88	2.94	1.00		
Mother's church attendance	2.20	1.08	2.08	1.14		
N		256		191		

sexual behavior are assumed to be causally prior to all subsequent variables. They are entered next. Sexual behavior of friends may be seen as a possible cause of school performance, deviant behavior, and noncoital sexual behavior, and are entered after the parental variables, after which school performance, deviance, and noncoital behaviors are entered. Since popularity may affect friendship choice or be affected by it, we have entered it after friends' behavior but before grades, deviance, and noncoital behaviors.

Attitude variables are attitudes at the time of interview. While previous attitudes might have affected school performance, deviant behaviors, and noncoital sexual experience, as well as selection of friends, we do not have attitudes measured at some prior time. For the purpose of this analysis, attitudes are taken as consequences of earlier variables in the model. Since attitudes about sex are more proximate to the behavior we wish to explain, they are entered into the model subsequent to the other, more general attitudes about self. The motivations for sex variables are assumed to be the conse-

quences of all other items, and are added next. Finally, the variable representing intentions to have sex in the future is added to the model last. As such, it is the intermediate link between an adolescent's motivation to become sexually active and have intercourse. That is, given the respondent's degree of motivation for sex, the intentions variable expresses what the adolescent plans to do about it.

If the predictor variable had no significant zero-order effect on the likelihood of first coitus, it was never a candidate for entry into the model. We based this decision on the need to make the model estimation procedure as manageable as possible and to maximize the number of cases on which the race-sex specific analyses are based.<sup>2</sup> When we use this strategy, the

<sup>2</sup> While this strategy does not detect variables whose effects become significant once other variables enter the model, we can check for such suppressor effects by adding variables whose effects are nonsignificant at the zero-order level during subsequent stages in the model-

model for each race-sex group contains different variables. This is inevitable when only a few variables are significant for more than one group.

A preliminary analysis showed zero order, race-sex interactions. This justifies developing separate multivariate models for each race-sex group. For black males, the time 1 virgin group was too small to permit a stable analysis of transition to coitus; therefore no analysis of black males is presented.

## RESULTS

### *White Females*

The far left column of table 2 gives zero-order effects on transition to intercourse. The remaining columns show the net effects of those variables as they are added sequentially to the model. Changes in the effects of variables previously entered can therefore be examined. The exposed diagonal gives the coefficients for each variable at entry. Given the causal priority assumptions of the entry order, these coefficients estimate the causal importance of each variable for the dependent variable.

Pubertal development, significant at the zero order, becomes nonsignificant upon the entry of age and remains nonsignificant throughout the sequential model-fitting process. Age, however, is strongly related to transition, regardless of the model specification. Controlling pubertal development for age is an over-control, since adolescents cannot develop without aging. The best interpretation of these data is that pubertal development is important, but there are age-graded effects that are important independent of pubertal development. Once age and pubertal development are taken into account, the attractiveness of the white female adolescent has no effect on her likelihood of making the intercourse transition.

Parental household structure is the only parental environment variable included in the white female analysis. Father's occupation is nonsignificant at the zero order. Mother's education and mother's sexual behavior at her daughter's age have significant zero-order effects but were dropped from the analysis because they cut the sample size due to missing maternal questionnaires. Later in the paper we present a separate full parental model.

The inverse effect of parental household structure is weakened and rendered nonsignificant by the introduction of school performance. While living with both original parents at the time of interview decreases the likelihood of a

white female making the intercourse transition, part of this effect may be mediated by the former variable's positive effect on school performance.

At the zero order, both the best same-sex friends' sexual behavior (ever had coitus) and the best opposite-sex friends' sexual behavior at time 1 are significant predictors. Regardless of the variables entered prior or subsequent to either of these variables, the friend variables exhibit significant effects on intercourse transition. As adolescents get older, they will have more chances to have friends who have had intercourse. Those who think intercourse would be more positive in its outcome might pick friends who have had intercourse. Those with more deviant behavior, poorer grades, and lots of petting experience may select friends more likely to have had intercourse. But with all these variables accounted for, the sexual status of both the best female friend and the best male friend are important predictors of the likelihood of intercourse transition for white females. These effects are, however, somewhat weakened by the entry of noncoital sexual experience, indicating that the best friend's intercourse behavior may partly operate by giving rise to greater sexual experimentation on the part of the adolescent.

School performance, deviant behavior, and noncoital sexual experience are also relatively unaffected by other variables' entry into the model. Engaging in other deviant behaviors increases the likelihood of making the intercourse transition, and grades in school have an inverse effect. Noncoital sexual experience has a very important additional effect. The more sexual experience short of intercourse a white female has had at time 1, controlling for her age, friends' experience, deviance, and school grades, the more likely she is to make the transition by time 2.

While a number of the attitudinal variables originally considered are significant at the zero order, only the general sexual permissiveness variable remains significant when entered into a stepwise model. The magnitudes of the zero-order effects of locus of control and future orientation are substantially reduced after the effects of the behavioral, environmental, and biosocial predictors are taken into account. The extreme reduction in the zero-order effect of premarital sexual permissiveness is no doubt due to its strong association with general sexual permissiveness ( $r = .46$ ).

With the exception of the thinking-about-sex variable, the motivation-for-sex predictors (the "turn-on" index, and subjective expected utility of sex) are relatively unaffected by prior controls. While not statistically significant at the .05 level, these two variables'  $p$  values range

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building procedure. This check was the final step in our analysis and no suppressor effects were found.

Table 2. Sequential Logistic Regression Models Predicting Transition to Intercourse Between Rounds 1 and 2: White Females (N = 280)

Variables	Net Effects																
	Zero-Order Effects	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	17
Pubertal development	.48**	.21	.21	.22	.25	.28	.30	.30	.16	.15	.11	.12	.12	.12	.11	.08	.06
Age	.73**	.63**	.59**	.59**	.50**	.49**	.50**	.37	.41*	.41*	.42*	.39	.39	.40	.45*	.46*	.49*
Attractiveness	.24*	.13	.13	.13	.13	.14	.14	.04	-.00	-.00	-.02	-.03	-.03	-.03	-.04	-.04	-.04
Parental household structure	-.70*			-.60*	-.73*	-.70*	-.58	-.61	-.58	-.56	-.53	-.50*	-.50	-.50	-.56	-.60	-.61
Same-sex friend's intercourse behavior	1.86**				1.71**	1.66**	1.54**	1.48**	1.23*	1.23*	1.25*	1.20*	1.19*	1.19*	1.30*	1.40*	1.33*
Opposite-sex friend's intercourse behavior	1.30**					1.30**	1.33**	1.32**	1.18**	1.20**	1.19**	1.24**	1.24**	1.24**	1.27**	1.28**	1.28**
School performance	-.65**					-.60*	-.60*	-.56*	-.74**	-.74**	-.75**	-.82**	-.82**	-.81**	-.91**	-.103**	-.96**
Deviant behavior	.89**							.74**	.60**	.59**	.57**	.52*	.52*	.53*	.54**	.51*	.53*
Noncoital sexual experience	.94**								.69**	.68**	.71**	.69**	.69**	.66**	.63**	.59**	.50*
Locus of control	-.21*								-.06	-.06	-.03	-.03	-.03	-.03	-.01	.02	.02
Future orientation	-.25*								-.15	-.15	-.14	-.14	-.14	-.13	-.13	-.17	-.16
General sexual permissiveness	.42**										.30*	.30*	.30*	.30*	.29*	.22	.14
Prenatal sexual permissiveness	.10**												.00	.00	.00	.03	-.04
"Thinking about sex"	.27**												.04	.04	-.05	-.13	-.18
"Turn-on" index	.16**														.14	.15	.16
Subjective expected utility of sex	.02**															.01	.01
Intention to have sex	.38**																.19
χ <sup>2</sup>	24.03	24.92	24.92	28.80	41.99	60.11	64.83	81.69	93.94	94.11	95.36	101.39	101.39	101.48	104.66	108.34	111.93
d.f.	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	17

Note: Coefficients represent the change in the log-odds of making the intercourse transition per unit's increase in the independent variable.

between .06 and .08 in columns 14 through 16 of Table 1. The "turn-on" index and the utility index, indicating the extent to which adolescents perceive that good or bad things will happen to them if they have intercourse, are positively related to intercourse transition. The utility index is quite negative, even for those making the transition to intercourse. Somewhat surprisingly, most of the variables that were significant prior to the entry of the attitude variables remain significant after the latter are included in the model. Only the general sexual permissiveness and the noncoital sexual experience variables appear to affect transitions indirectly by giving rise to the motivation for sex. Their effects are reduced when subjective expected utility is entered.

Finally, the effect of the intentions-to-have-sex variable, when entered last, is substantially reduced from its zero-order relationship to transition. This suggests that much of the original effect of the intentions variable is spurious, caused by other variables in the model being related to both transition to intercourse and future intentions to have intercourse. Perhaps the other variables establish an opportunity structure for becoming sexually active that is independent of intentions. Regardless, the *p* value associated with the intentions coefficient in column 16 is .06, barely above the usual criterion for statistical significance. Moreover, entering the intentions-to-have-sex variable brings about reductions in the coefficients of female friends' sexual behavior, noncoital sexual experience, and general sexual permissiveness, suggesting that these variables were partly precursors of intentions to have intercourse.

As a final step in our analysis of white females' transition to intercourse, we reran all models using the same general plan as before but deleting variables from the next model whose significance level was above .10 in the previous model, until a final model was achieved. The results of the final model are shown in column 17. We removed 8 of the 17 variables, including 6 attitudinal variables, attractiveness, and pubertal development. Intentions is highly significant in the final model, suggesting that its correlation with the attitudinal variables weakened it in the earlier models. Otherwise, the interpretation of the results is not changed much by deleting nonsignificant variables. We reemphasize that the coefficients at entry, not in the final model, are the causal effects of variables.

### White Males

Table 3 presents the results for white males. Its most striking feature is that, compared to white females, relatively few variables are significantly related to intercourse transition, even at the zero order. In contrast to our cross-sectional models (Billy and Udry 1984), in which pubertal development strongly correlates with nonvirginity but age makes no additional contribution, in the transition model neither age nor pubertal development is a significant zero-order predictor. Among the peer environment variables, only popularity with the opposite sex is significant. Unlike white females, neither same-sex nor opposite-sex friends' intercourse behavior is significant in predicting transition. This sex difference may be accounted

Table 3. Sequential Logistic Regression Models Predicting Transition to Intercourse Between Rounds 1 and 2: White Males (*N* = 264)

Variables	Zero-Order Effects	Net Effects						
		1	2	3	4	5	6	7
Popularity with opposite sex	.16**	.15*	.11	.12	.13*	.13*	.13*	.14*
Deviant behavior	.25*	.20	.16	.09	.08	.05	.02	
Noncoital sexual experience	.51**		.32	.25	.23	.17	.13	
General sexual permissiveness	.31**			.25*	.21	.16	.13	
Premarital sexual permissiveness	.06*				.03	.01	.01	
Subjective expected utility of sex	.02**					.01	.01	
Intention to have sex	.21**						.09	.20*
$\chi^2$		11.43	14.05	19.23	19.95	21.73	22.91	19.58
d.f.		2	3	4	5	6	7	2

Note: Coefficients represent the changes in the log-odds of making the intercourse transition per unit's increase in the independent variable.

\* *p* < .05.

\*\* *p* < .01.



for by the fact that in this sample, females have much more accurate information than males concerning their friends' coital behavior (Wilcox and Udry 1986).

School performance is not a significant predictor. Deviance and noncoital sexual experience are significant at the zero order and, therefore, candidates for inclusion in the analysis. When we control for popularity with the opposite sex, however, both the deviant behavior and noncoital sexual experience variables are weakened and rendered nonsignificant. As indicated in column 2 of Table 3, the effect of popularity with the opposite sex itself is weakened when deviant behavior and noncoital sexual experience are included in the model. This suggests that these three variables are interrelated rather complexly in predicting a white male's transition to intercourse. Given that all three of the variables were measured at time 1, their order of entry into the model is arbitrary and nothing can be inferred about their causal connectedness. We should note, however, that both deviant behavior and noncoital sexual experience are far weaker predictors of the intercourse transition for white males than for white females.

Among the attitudinal variables, only general sexual permissiveness is significant when entered into a stepwise model. Its inclusion further reduces the effect of deviant behavior and noncoital sexual experience, suggesting that any effect of deviance or sexual experience on intercourse transition is partly due to their creation of more permissive sexual attitudes.

Subjective expected utility is significant at the zero order. When it is entered into the model, it further reduces the strength of most other variables entered earlier, but is itself not significant. Nor is intention to have sex a good predictor of actual behavior between rounds 1 and 2. The magnitude of the intentions-to-have-sex effect is reduced from a zero order of .21 to a nonsignificant level of .09 in column 6,

indicating that most of the relationship between future intentions and transition is explained by other variables in the model, especially popularity with the opposite sex. It appears, then, that a white male's likelihood of making the intercourse transition may be more a function of his opportunity to do so than his intentions to do so.

We reran all the male models, using the same general plan as for the females for deleting variables above the .10 significance level. The final model is shown in column 7. Only popularity with the opposite sex and intentions to have sex are significant, again indicating that in earlier models "intentions" was weakened by its relationship to the other attitudinal variables.

### *Black Females*

The black female model, presented in Table 4, is much simpler because fewer variables are significant at the zero order. The effects of the parental environment variables are untestable because although there were more than 100 virgins at time 1, there were fewer than 50 maternal questionnaires, and missing values lower *N* below the level of practicality. The noncoital sexual experience factor is unusable for blacks because the extracted factor is weak (eigenvalue < 1.0) and has no predictive power. School performance and deviance are not related to transition to intercourse, nor are friends' sexual behavior and popularity with the same and opposite sex.

While both age and pubertal development are significant at the zero order, the two-variable model containing both is not significantly better than pubertal development alone, but is much better than age alone. Pubertal development is, therefore, the important variable predicting intercourse transition for black females, with no additional contribution of age. This contrasts with the exact opposite situation for white females.

None of the attitudinal variables significantly

Table 4. Sequential Logistic Regression Models Predicting Transition to Intercourse Between Rounds 1 and 2: Black Females (*N* = 86)

Variables	Zero-Order Effects	Net Effects			
		1	2	3	4
Pubertal development	1.10**	1.01**	1.04**	1.10**	1.17**
Age	.42*	.18	.18	.12	
Subjective expected utility of sex	.02*		.02*	.02*	
Intentions to have sex	.21*		.19	.20	
$\chi^2$		14.35	20.39	23.45	20.10
d.f.		2	3	4	2

Note: Coefficients represent the change in the log-odds of making the intercourse transition per unit's increase in the independent variable.

\*  $p < .05$ .

\*\*  $p < .01$ .

affects the likelihood of making the intercourse transition. Subjective expected utility of sex, however, does have a positive effect on the dependent variable, but its entry into the model does not weaken the effect of pubertal development. As is the case for white females, the utility index is quite negative, even for those black females making the transition to intercourse. The intentions-to-have-sex variable is significant at the zero order. Its magnitude is only slightly reduced, and it is barely nonsignificant when entered into the model last. The final model for black females (column 3) contains only four variables: pubertal development, age, utility, and intentions. With the exception of age, their coefficients are quite stable from the zero order to the combined model. This implies that intentions to have sex in the future are not explained by pubertal development or subjective expected utility, and that these two variables' effects on intercourse transition are independent of intentions.

Deleting variables from the black female model that were not significant at the  $p < .10$  level removed age and utility and strengthened pubertal development (column 4). Pubertal development comes through as a very strong predictor, with one standard deviation increase in pubertal development causing a more than threefold increase in the probability of transition to intercourse. This means that a virgin black girl who was one standard deviation above the mean in pubertal development at time 1 was almost 10 times as likely to make the transition to intercourse as one who was one standard deviation below the mean. Her intentions to have sex have a nearly completely independent effect, but one that is trivial by comparison to pubertal development. Because the sample size for black females is only 86, we persist in interpreting the final coefficient for intentions, since it is not reduced from the zero order by pubertal development, and since it approaches conventional significance levels, even with the small sample size.

#### *Tests for Homogeneity by Race and Sex*

To determine whether there were overall differences in the prediction equations by race and sex, we ran a set of prediction equations that included all variables significant at the zero order for any race-sex group (18 variables), dummies for race or sex, and all interaction terms by race or sex. Comparing these models with a main-effects model indicates that the equations are not homogeneous by race or sex. (For race effect among females,  $X^2 = 47.22$ ; for sex effect among whites,  $X^2 = 35.43$ , in each case, d.f. = 19, both significant at  $\alpha = .05$ .) We may conclude that there are overall

differences in the determinants of initiation of coitus by race among females, and by sex among whites. The race difference among females is especially striking.

#### *A Parental Control Model*

Because of the theoretical and practical interest in social controls originating with the parent, we have constructed a special parental model with eight variables theoretically related to parental control. This model has a reduced sample size from that of the main model, since some of the variables are only available on the parental questionnaire. This model requires entering every pertinent variable in a theoretically defensible order, stepwise, irrespective of its zero-order relationship to coital transition. Initially, pubertal development, age, and attractiveness were entered, then the parental variables. Variables "lower in the model" (which come after the parental variables) are not changed in their relationships in any significant way, whether the restricted parental sample or the full sample is used; on the restricted sample, subsequent variables are not changed whether or not the parental variables are in the model. We do not offer a parental model for blacks because of the small sample size. Parental models are shown for boys in Table 5 and girls in Table 6.

For boys, only mother's education is significant (.05 level) at the zero order. It is almost significant when entered at step 4, and is significant at all subsequent steps, with a stable coefficient. No other variable approaches significance at any step. The overall model for boys does not reach anything close to significance at any step. In the final model, mother's education remains the only significant variable.

For girls, mother's education, parental marital status, and mother's adolescent sex are significant at the zero order. The education coefficient is about the same when entered on step 4 as at the zero order, and almost significant. The coefficient remains stable during the remaining steps of the model. Parental marital status is not significant at entry step 6, because it is related to mother's education, entered earlier. Mother's adolescent sex is not significant when entered at step 7 because it is related to mother's education and parental marital status. Because the mother reported the variable as of her daughter's age, it also splits the variance related to age. No variables are significant in the final model.

We conclude that highly educated mothers, mothers who are married to and living with the fathers, and mothers who were less sexually active as adolescents have daughters with reduced probability of coital transition. The first two findings are consistent with existing literature, and the last is a finding unique to this

Table 5. Logit Regression Coefficients Predicting Transition to Coitus for Time 1 Virgins: Parental Model for White Females ( $N = 252$ )

	Model Number										
	1	2	3	4	5	6	7	8	9	10	11
Pubertal development	.42*	.26	.26	.29	.31	.30	.27	.26	.27	.28	.28
Age	.48**	.38*	.36	.31	.31	.28	.19	.26	.27	.26	.25
Attractiveness	.14		.10	.12	.12	.10	.09	.09	.09	.09	.09
Mother's education	-.09			-.09	-.10*	-.10*	-.11*	-.10*	-.10*	-.10*	-.10
Father's occupation	-.02				.16	.17	.17	.20	.20	.19	.19
Household structure	-.73*					-.57	-.43	-.44	-.44	-.40	-.41
Mother's sexual experience	.32**						.22	.25	.25	.24	.24
Mother's control	.04							.14	.15	.14	.14
Mother's religiousness	.10								-.04	.07	.06
Mother's church attendance	-.13									-.14	-.15
Mother's premarital permissiveness	.00										-.03
$\chi^2$		10.6	11.0	14.5	15.3	17.9	20.9	22.7	22.7	23.3	23.3

\*  $p < .05$ .\*\*  $p < .01$ .

study. Surprising is the lack of effect of the mother's control behavior, religious behavior and attitude, and attitude toward premarital sex for the sexual behavior of either daughters or sons.

## DISCUSSION

Returning to our conceptual model in Figure 1, we find that for white females, every component of the model is explanatory, with the exception of attractiveness. For white males, few components of the theoretical model predict transition. There are no observed effects of social control components. This might lead to the conclusion that white female sexual behavior is well understood, while both white male and black female sexual behavior are poorly understood. However, in the case of white females versus white males, the results take on a different

meaning when they are combined with our study of hormone influences on the sexual behavior of whites.

In another part of this project, reported elsewhere (Udry et al. 1985), we determined that the sexual behavior of adolescent white males is strongly influenced by male hormones (androgens) on every aspect of sexual behavior and motivation we examined, including coitus. Results of a parallel study of white females (Udry et al. 1986) showed that neither male nor female hormones had any effects on female coital behavior. However, female sexual motivations and noncoital sexual behavior appear to have a foundation in androgenic hormones. For both males and females, these hormone effects are independent of and do not work through pubertal development, and are not related to the fact that hormone levels increase with age. Therefore, the hormone effects may be consid-

Table 6. Logit Regression Coefficients Predicting Transition to Coitus for Time 1 Virgins: Parental Model for White Males ( $N = 191$ )

	Model Number										
	1	2	3	4	5	6	7	8	9	10	11
Pubertal development	.23	.24	.24	.25	.24	.25	.24	.24	.28	.26	.27
Age	.11	-.01	-.01	-.07	-.07	-.06	-.09	-.09	-.04	-.04	-.04
Attractiveness	.02		.01	.01	.02	.02	.00	.00	.02	.02	.04
Mother's education	-.10*			-.10	.11*	-.11*	-.11*	-.11*	-.12*	-.12*	-.12*
Father's occupation	.06				.17	.18	.17	.17	.15	.16	.17
Household structure	.07					-.12	-.06	-.07*	-.06	.12	.14
Mother's adolescent sexual experience	.10						.08	.08	.08	.08	.07
Mother's control	.03							.03	.07	.09	.10
Mother's religiousness	-.16								-.28	-.12	-.08
Mother's church attendance	-.22									-.24	-.19
Mother's premarital permissiveness	.11										.10
$\chi^2$		1.7	1.7	5.6	6.4	6.5	6.8	6.9	9.4	10.8	11.2

\*  $p < .05$ .

ered to have a biological effect on libido and cannot be considered to work through social interpretations of age or pubertal development.

For black females, the most notable finding is that pubertal development is the only strong predictor of transition to coitus. Our hormone analysis for girls (Udry et al. 1986) eliminates the possibility of interpreting this as anything but an attractiveness variable. No effects of social controls are observed for black females.

The absence of observed effects of social controls for black females and white males, and the strong social effects for white females must come as a surprise to most sociologists. Effects of parents and friends are central to most discussions of determinants of adolescent sexual behavior. Furthermore, the absence of effects contradicts the research of others. Two considerations must be kept in mind when evaluating these contradictions. First, no previous studies have had research designs in which the measurement of the social effects of friends and parents were both based on behavior reports of parents and friends and based on measures taken before the transition to coitus. This favors the present findings against any previous findings. Second, all previously reported relationships are based on studies of older adolescents. The influence of social controls may be different at different ages. Almost all transitions reported in this paper occurred between ages 12 and 16. The strong sex differences reported in this study are in contrast to Thornton and Camburn (1983), who report no sex differences in family effects.

The findings of the hormone analyses combined with the findings from the analyses presented above lead to our overall conclusions. The initiation of sexual behavior, including coitus, of early adolescent males is strongly differentiated by the effects of male hormones (androgens), but the effects of social controls, if they exist, are so uniform that they do not have explanatory power in microanalysis. Sexual motivation and noncoital sexual behavior of early adolescent white females are substantially influenced by androgens, but their coital behavior is primarily differentiated by social control processes, not hormones.

The findings for black females are illuminating as much by the negative findings as by the positive findings. Pubertal development (an attractiveness dimension) is the best predictor of transition to coitus. The absence of family, school, and friendship effects is puzzling. The data suggest that the high rate of early sexual activity of black girls compared to whites may be related to differences in the experience of blacks and whites that are not represented in our models.

The absence of social control effects in a microanalysis such as ours does not mean that

there are no social controls on the sexual behavior of black females or white males. There may be "effects" (in a different sense) that are uniform across all members of the group, pressuring toward or against certain behaviors, but that do not differentially affect individuals and, therefore, are invisible in a microanalysis. Males are often said to experience strong peer pressure to engage in coitus. Nothing in the present analysis contradicts this.

Why the sex differences in social effects and hormone effects? We pose the following hypotheses for further research: (1) The social controls and normative environments that girls encounter are highly differentiated, while the environments that boys encounter are more uniform, producing microeffects for females but not for males; and (2) The effects of hormones on males' behavior are much stronger than on females because, as males mature, their androgen levels go up by a factor of 10 to 20, while, in females, androgen levels hardly double, with males and females starting from the same prepubertal levels. In males, the hormone effects may overwhelm the social controls.

We urge caution in accepting our results and interpretations. While our research design is superior to previous studies in most respects, and the number of domains of behavior examined is extensive, there are important limitations: (1) The study was conducted in a single city; (2) The black female sample is small; (3) The hormone results are from cross-sectional studies that need to be confirmed from panel designs; and (4) The conclusions apply only to early adolescence and cannot be generalized to coital transitions occurring earlier or later.

## APPENDIX

### *Measurement of Variables*

*Transition to intercourse.* At time 1, after defining "to have sex" explicitly, the respondent was asked to indicate whether he or she had had sex. The response categories were: (1) I have never had sex; (2) I have had sex 1 or 2 times in my life; and (3) I have had sex more than 2 times in my life. At another point in the round 1 questionnaire, along with the items in the deviance scale, the respondent was asked again if he or she had ever had sex. These two questions were repeated at time 2. If, at time 1, the respondent indicated having had sex on *either* question, he or she was coded as nonvirgin. All who were scored as having had sex at time 1 were eliminated from the analyses in this paper. A person is considered to have made the transition to intercourse between round 1 and round 2 if at time 1 he or she answered negatively to both questions but at time 2 answered positively to either question. A small number of discrepant cases were eliminated from analysis (those who were scored "yes" at time 1 and "no" at time 2).

*Age.* Age was calculated to two decimal places by subtracting reported birth date from interview date.

**Pubertal development.** Respondents rated themselves on level of pubertal development as registered through a battery of discrete items. These included a set of Tanner-type line drawings (Tanner 1962) representing the degree of pubic hair and breast or genital development. Each person also indicated on separate items the development of body hair, facial hair, voice change, body shape change, and menarche, as appropriate by sex. These items were factor-analyzed separately by sex, and a single factor was extracted. Factor scores were assigned individuals on the basis of the factor weightings of the items. This score has a mean of zero and a standard deviation of one. The self-measurement of pubertal development was validated in a separate study (Morris & Udry 1980), in which pediatricians rated adolescents on a battery of nearly identical items for which adolescents had already rated themselves. Factor scores from physicians and adolescents correlated  $r = .74$  for males and  $.82$  for females.

**Attractiveness.** Attractiveness of the respondent was rated by the interviewer on a six-point scale ranging from "not very" to "very."

**Socioeconomic status of family of origin.** Two measures of SES are used: (1) mother's report of her years of completed education; and (2) father's occupational status. Father's occupational status is derived from a scale based on mother's report in round 1, or if missing, the adolescent's report on the same scale from round 2. Occupations were classified according to the following metric: (5) professional/technical; (4) managers, officials, and proprietors; (3) clerical, sales, and kindred; (2) skilled workers; and (1) unskilled workers.

**Mother's sexual experience when adolescent's age.** Mothers reported their sexual experience at the same age as their adolescent on the following scale: (1) held hands; (2) necked; (3) petted; and (4) had sexual intercourse.

**Parental household structure.** The adolescent was given a score of one if he or she was living with both original parents at round 1, otherwise zero.

**Same-sex and opposite-sex friends' intercourse behavior.** Each respondent was asked to write down on a piece of paper the names of three friends of each sex, with best friend first. The interviewer and respondent looked at a list of pupils in the school and put a code number for each friend in the respondent's questionnaire. Some respondents named persons not in the school and were asked to replace these friends with persons in the school. Friends' questionnaires were matched, and whether the adolescent's best friend of each sex had ever had intercourse was created as a variable.

**Adolescent's popularity with the same sex and opposite sex.** Popularity is defined as the number of times a respondent was named as a best friend by other respondents in the study. Two separate measures are used: (1) popularity with the same sex; and (2) popularity with the opposite sex.

**School performance.** Respondents reported whether their school grades were: (1) below average; (2) average; or (3) above average.

**Deviant behavior.** Deviant behavior was measured by a four-item scale of minor infractions. Respondents were asked had they ever: (1) smoked cigarettes; (2) drunk alcoholic beverages; (3) cheated on a test; and (4) driven a car without a license. A summative index score was assigned to each respondent.

**Noncoital sexual experience.** Respondents were asked to indicate whether they had ever engaged in a series of

heterosexual behaviors ranging in intimacy from holding hands through manual manipulation of bare genitals. A factor score was constructed from these items using the same strategy as for pubertal development.

**Self-satisfaction.** A summative index of self-approval was constructed from the following four questions: (1) How well can you do the things you would like to do? (2) How much do you respect yourself? (3) How good do you feel about how you will develop as a person in the next few years? and (4) On the whole, how happy are you with yourself? Each question had three response categories ranging from "not too" to "very."

**Locus of control.** Two items from the Rotter scale of internal-external locus of control were used to create a summative index. Respondents rated themselves on a five-point scale ranging from "strongly agree" to "strongly disagree" for each of the following statements: (1) Most of the time what happens to me is my own doing; and (2) When things turn out well for me, I usually feel that I can take much of the credit.

**Future orientation.** A summative scale was constructed based on two items indicating future orientation. Respondents rated themselves on a four-point scale ranging from "very often" to "never" for the following questions: (1) How often do you think about what your life will be like one year from today? and (2) How often do you think about what it will be like to be 21 years old?

**Religiousness.** The adolescent responded to a four-category, single item, "How important is religion to your daily life?" Choices ranged from "not important" to "very important."

**General sexual permissiveness.** Sexual permissiveness was measured by a two-item summative index created from the following statements: (1) Only adults should have sex; and (2) Only married couples should have sex. Each statement had the three response categories of "I agree," "I don't know," and "I disagree."

**Premarital sexual permissiveness.** This is a seven-item summative scale in which on each item the respondent indicated approval ("strongly agree" to "strongly disagree") of statements concerning the importance of restricting sex to marital or love relationships. (Sample item: If she asked me, I would tell my sister not to have sex before marriage.)

**Sexual awareness.** Sexual awareness/motivation is measured by two variables: (1) thinking about sex; and (2) a "turn-on" index. For the former, respondents indicated how frequently they thought about sex, from "several times a day" to "almost never." For the latter, respondents checked a list to indicate what things in their environment aroused them sexually (music, reading, movies, nude pictures), and a summative index was constructed.

**Subjective expected utility of sex.** Respondents indicated how likely they considered 20 consequences if they had intercourse, and how much they would like or dislike each consequence. The probability score for each item was multiplied by the score for how much they would like the consequence, and a summative index was prepared.

**Intentions to have sex in the future.** A summative index indicating motivation for sex in the future was constructed using three questions: (1) How much do you think you would like to have sex in the next year? "dislike very much" to "like very much"; (2) Do you intend to have sex in the next year? "no," "don't know," "yes"; and (3) How likely is it you will have sex in the

next year? "sure it wouldn't happen" to "could happen for sure").

**Maternal control.** Maternal control behavior was measured by asking the mother to indicate yes/no to a set of seven questions concerning whether she enforces restrictions on night-time weekend hours, who the child dates, who the child goes around with, what clothes the child wears, bedtimes, whether she is very strict with the child's behavior, and whether the child is allowed to make decisions most of the time. A sum of these items constitutes a maternal control scale.

**Maternal religiosity.** Mother responded to a four-category single item, "How important is religion to your daily life?" Choices ranged from "not important" to "very important."

**Maternal church attendance.** Mother responded to a four-category single item, "how often have you attended church services in the last year?" Choices ranged from "less than once a month" to "more than once a week."

**Mother's premarital sexual permissiveness.** A two-item scale asking the types of relationships the mother considers appropriate for coitus (married only, adult only). Disagreement was scored as permissive, and the number of disagreements was summed for a score.

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## RESEARCH NOTE

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### IS THERE AN ASSOCIATION BETWEEN GENDER AND METHODS IN SOCIOLOGICAL RESEARCH?\*

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*Feminist scholars have proposed that two types of links exist between research methods and gender. Female scholars have been thought to be more likely than males to choose qualitative methods because such methods are compatible with relational and emotional skills stereotypically associated with women. Qualitative approaches also have been thought to be especially appropriate for study of gender issues and women's experiences and to be an effective strategy for correcting androcentric biases in construction of social theory. We examine articles in 10 sociology journals in 1974-83. Most articles have been quantitative, but female authors have used qualitative methods more often than males. Writing about gender increased rather than decreased the likelihood of having used quantitative methods for both women and men. We suggest that papers focusing on gender and also using qualitative methods represented double nonconformity and hence were unlikely candidates for publication in mainstream journals.*

#### METHODS AND GENDER ISSUES

A frequent, but largely untested, assertion in the social sciences is that there is a systematic association between gender and methods. Two connections between gender and methods have been proposed. First, some writers have argued that female researchers prefer qualitative approaches. Qualitative methods involve prolonged, sometimes emotional ties with research subjects. They draw upon women's presumed greater skills and interests in communal and relational aspects of social life (Bernard 1973; Mackie 1985; Roberts 1981; Sherif 1979; Smith 1974, 1979; Stanley and Wise 1983). Qualitative methods also demand less abstraction from context and statistical analysis, work styles thought to be more compatible with males' rather than females' skills and preferences (Bakan 1972; Carlson 1972; Gilligan 1982).

A second rationale for a link between gender and methods is the contention of feminist

theorists that qualitative methods are particularly appropriate where gender and women's issues are topics of inquiry. Important dimensions of women's lives, more so than men's, are contained within private, emotional realms. Significant events in women's lives are subtle and context-bound, the very sorts of phenomena that are better illuminated by qualitative than quantitative approaches. Qualitative methods, such as participant or nonparticipant observation or intensive interviews, are appropriate for inquiries into unexplored topics in social life in which women rather than men are central actors (e.g., childrearing, neighborhood friendship groups, ties with kin) (See Bernard 1973, 1975; Daniels 1975; DuBois et al. 1985; Oakley 1974; Smith 1974, 1979; Unger 1983).

Bernard (1973) argues that men live in a "cash nexus" world in which important indicators of social status and social ties can be usefully quantified and compared across contexts (e.g., salary or job status). In contrast, more of women's lives are spent in what Bernard terms the "status nexus" world, where prospects for cross-situational comparison and quantification are limited. It is more difficult, for example, to develop a meaningful scale of love for one's child or devotion to one's aging parents than to measure and compare job status or salary. Quantitative methods that require abstraction from context work poorly when

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studying subtle facets of men's lives (for example, male friendships). Since women in contemporary society spend more time than do men in these complex and emotional realms of social life, quantitative methods provide less faithful portraits of their experiences and perspectives. Context-bound, particularistic phenomena, Bernard argues, "slip through the matrix" of quantitative analytical strategies, which typically attempt to minimize the number of variables to maximize the number of comparable cases. Qualitative approaches also have been viewed as consistent with feminist social scientists' use of emotion and self-reflection as data (Cook and Fonow, 1985).

A related issue is the argument raised by several writers that qualitative methods hold the greatest potential for correcting "androcentric" biases in sociological research. (See Griffin 1986; Stacey and Thorne 1985 for two recent statements). Many social theories have been developed with reference to men alone. When scholars have applied male-derived theories to women's experiences, frequently the fit is poor. Griffin, for example, found that theories of relationships between British working-class boys' orientations toward school and their later job placement did not accurately describe this transition for working-class girls. The girls were more difficult than the boys to classify as members of "pro" or "anti" school cultures. There also was less association between girls' schooling attitudes or performances and their later jobs than was the case for boys. Griffin used qualitative methods (nonparticipant observation combined with intensive interviews) to construct a theory more consistent with the girls' experiences.

Calls for "recentering" knowledge about women—building theories based on women's experiences rather than extrapolating from theories derived from study of men—often have been coupled with an admonition to return to inductive, qualitative approaches, at least in the early stages of the inquiry (Cook and Fonow 1985; Stacey and Thorne 1985). This perspective seemingly does not preclude quantitative analyses of women's lives at later stages, once the relevant empirical and theoretical concerns have been defined with reference to women's rather than men's experiences.

The association between gender and methods is complicated by the fact that qualitative and quantitative methods hold unequal prestige in social science and science generally. Quantitative methods, "the methods of the male-stream" in Griffin's (1986) words, have greater prestige (see also Bart 1971; Bernard 1973; Keller 1985; Reinharz, 1979; Smith 1974, 1979). Such methods are regarded as more objective and scientific than interpretive methods, a perspec-

tive challenged by feminist scholars as desirable or even possible (Bart 1971; Keller 1985; Rubin 1977, Smith 1974). As is the case for many other phenomena in a gender-stratified society, techniques associated with men and male lifestyles are accorded greater worth, in part because males dominate the mechanisms by which scholarly work is evaluated. They, more so than women, are the gatekeepers who make the critical decisions about what is printed, what is seen by others, and what is legitimated as creditable scholarly work (Spender 1981; Stanley and Wise 1983). Even if women and/or scholars conducting research on women's lives prefer qualitative approaches or believe them to be more appropriate for their topics, they may be pressured to carry out analyses in other forms if they want their research to be seen. This may be especially true when the desired outlets for publication are leading journals in sociology, because such publications become display cases for what is regarded as important and valued within a discipline (see Mackie 1985). Methodological conformity may be a stronger demand of the gatekeepers when researchers take on nontraditional topics, such as gender.

#### ANTICIPATED LINKS BETWEEN METHODS AND GENDER

Feminist critics suggest that we might expect several types of association between method and gender. These associations also will be influenced by norms and practices surrounding the publication process itself, and the perspectives on methods, gender, and female scholars of those who dominate that process.

Despite substantial speculation about the relationships between gender and methods in sociology, surprisingly little empirical work has addressed the issue. Carlson (1972) found a relationship between author gender and propensity to use quantitative methods among psychologists. This study found that, although published work in psychology was skewed toward quantitative analysis, men used quantitative approaches more frequently than did women.

In a more recent study of sociological publishing, Mackie (1985) found an increasing tendency over time toward quantitative analysis in articles published in five major journals. Her study of published articles by women and by men in three years (1967, 1973, and 1981) found that, despite this general pattern, women published more communal (nonstatistical) articles than men did. Mackie also found that sociologists who published in gender-oriented interdisciplinary journals used communal methods more frequently than sociologists publishing in sociology journals. She interprets this as indicating that women conformed to perceived

methodological demands of mainstream gatekeepers in sociological publications, but used qualitative methods when publishing in gender-oriented journals. Unfortunately, Mackie offers no data about whether the same scholars published in both sources. It is possible that a different groups of scholars published in each outlet.

Neither Carlson nor Mackie explored directly the relationship between article topic and method. Nor did these studies examine the relative impact of author gender and article topic on method choice in a multivariate framework.

This paper empirically tests three questions implicit in the mostly nonempirical writing on the relationships between gender and method in published sociological work. These are: (1) Women scholars are less likely than men to use quantitative methods in research; (2) Works focusing on gender or women's experiences are less likely than other research to use quantitative methods; and (3) Works focusing on gender written by women are less likely than other works to use quantitative methods.

We were also interested in whether there had been changes over time in women's and men's use of quantitative methods when writing about gender or other topics. Mackie (1985) reported an increased tendency in 1981 compared to 1967 and 1973 for women and men to use quantitative methods. However, she did not use multivariate methods to explore relationships between author gender, topic, and time. As male and female sociologists received more sophisticated quantitative methods training, and as more data were collected on gender issues and women's lives, quantitative analysis by women and of women's lives might have increased. Women in the past decade have increasingly moved into domains of social life, such as jobs and higher education, where quantitative techniques can be meaningfully applied to study their experiences. Also, if quantitative papers were easier to publish in major sociological journals, scholars might have used these methods to maximize their chances of publication.

## SAMPLE AND METHODS

To address these questions we drew a stratified random sample from the population of 3,674 full-length articles published in 10 major sociology journals in 1974–83.<sup>1</sup> The population

included all full-length articles (excluding book reviews, commentaries, and research notes) published in *American Journal of Sociology* (AJS); *American Sociological Review* (ASR); *Journal of Health and Social Behavior* (JHSB); *Pacific Sociological Review* (PSR, subsequently retitled *Sociological Perspectives*); *Social Forces* (SF); *Social Problems* (SP); *Social Psychology Quarterly* (SPQ, formerly titled *Sociometry*); *Sociological Quarterly* (SQ); *Sociology of Education* (SOE); and *Work and Occupations* (WO). These included articles in all publications sponsored by the American Sociological Association, with the exception of *Contemporary Sociology*, a journal of book reviews, and *American Sociologist*, which was not published for the entire period covered by our review. It also included reputable regional and specialty journals. (The data are described in Ward and Grant 1985.)

For the current study we drew a stratified random sample of 214 articles from each of four groups:

1. Articles focused on gender, sex roles, or sexuality that had female solo or first authors (henceforth female gender articles).
2. Articles focused on gender, sex roles, or sexuality that had male solo or first authors (male gender articles).
3. Articles focused on other topics that had female solo or first authors (female nongender articles).
4. Articles focused on other topics that had male solo or first authors (male nongender articles).

The sample included all cases of female gender articles, the category with the smallest number of cases ( $N=214$ ). We then randomly selected an equivalent number of cases from each of the other three categories. This resulted in taking about half the female nongender articles, about one-third the male nongender articles, and about one-tenth the male nongender articles, yielding a total of 856 cases.

Criteria for classifying articles into these four categories are discussed in detail in Ward and Grant (1985). This previous study, based on all 3,674 full-length articles published in the 10 journals in the decade, revealed a systematic association between gender of author and topic. Over the 10 years, women scholars were more than twice as likely to be solo authors of gender articles than of published articles generally (45 versus 20 percent). For coauthored work, they were nearly twice as likely to be first authors of gender articles as compared to all articles (39

<sup>1</sup> In Ward and Grant (1985), we reported 217 cases in the female gender-article category. In drawing the sample for this analysis, we eliminated one of these cases which had been misclassified and actually belonged in category two. We also deleted two articles, which, upon reexamination, we decided constituted editorial commen-

tary or book reviews rather than traditional journal articles.

Table 1. Research Method by Type of Article for 856 Papers Published in 10 Sociology Journals in the Period 1974-83

Article Type	% Quantitative	% Qualitative	% Other	Total	
				N	(%)
Female gender <sup>a</sup>	79.4	14.5	6.1	214	(100)
Male gender <sup>b</sup>	91.1	3.8	5.1	214	(100)
Female other <sup>c</sup>	62.7	20.6	16.8	214	(100)
Male other <sup>d</sup>	71.0	10.3	18.7	214	(100)

<sup>a</sup> Female gender article = article solo or first-authored by a female that focuses on topics of gender, sex, sex roles, or sexuality.

<sup>b</sup> Male gender article = article solo or first-authored by a male that focuses on topics of gender, sex, sex roles, or sexuality.

<sup>c</sup> Female other article = article solo or first-authored by a female not focusing on issues delineated above.

<sup>d</sup> Male other article = article solo or first-authored by a male not focusing on issues delineated above.

versus 21 percent). Coauthored articles were classified by gender of first author.<sup>2</sup>

Sampled articles were classified as quantitative, qualitative, or other. "Other" articles usually were not data-based. Most were theoretical papers, critiques, or literature reviews. Papers using statistical analyses were classified as quantitative, and these required some statistical training to prepare and comprehend. A few employed simple methods, such as chi-square analyses, but most used multivariate techniques. Qualitative articles were data-based reports that did not quantify data beyond frequency counts. Included in this category were nonparticipant observations, participant observations, case histories or case studies, intensive interviews, document, textual, or historical analyses, and sociolinguistic approaches. These articles usually employed systematic classification but presented data primarily in the form of quotations or verbal summations. Less than 2 percent of the articles were judged to employ both qualitative and quantitative approaches. They were classified in the "other" category.

## RESULTS

We first present descriptive data on methods type used in articles sampled from the four categories. We then break down the data into two time periods, early (1974-78) and late (1979-83). Finally, we present results of logistic regressions to examine the combined effects of author gender, article topic, and time period on methods choice.

Table 1 shows percentages of articles across the four categories that used quantitative, qualitative, or other methods. Table 1 aggregates data from all journals and years. The majority of papers in all categories used quantitative methods. Comparisons across gen-

der of author show that, regardless of article topic (gender or nongender), women were less likely to use quantitative methods and more likely to use qualitative approaches than were men. About 71 percent of men's nongender articles, compared to 63 percent of women's, were quantitative. About 91 percent of men's gender articles, but 79 percent of women's, were quantitative. Women used qualitative analysis nearly twice as often as men in the nongender article category and more than three times as often in gender articles.

Comparisons of methods across article type (gender or nongender) show an unanticipated pattern. For both women and men, gender articles were more apt to be quantitative than were nongender articles. The difference was stronger for men than for women. A little more than 91 percent of men's gender articles, compared to 71 percent of their papers on other topics, were quantitative—a difference of 20 percent. For women, the difference was slightly smaller, but in the same direction, with 79 percent of gender articles but only 63 percent of papers on other topics using quantitative approaches.

Within article type, there is little difference by author gender in proportions of "other" articles. The "other" category, it should be recalled, contained mostly articles which were not data-based. For nongender articles, women and men had similar proportions of such published papers over the 10 years (17 versus 19 percent, respectively). As table 1 also shows, gender articles using "other" methods were rare, regardless of whether their authors were women (6 percent) or men (5 percent). This might have occurred because gender articles represented a new field during the time of our analysis.

Table 2 disaggregates the data into two time periods, early (published 1974-78) and late (1979-83). For all articles, quantitative approaches were used slightly more often in the late than the early years. The greatest difference, about a 9 percent increase, appears for gender articles by men. In other categories, the

<sup>2</sup> Classification by gender of first author is supported by recent work by Mackie (1985), who found only a small proportion of coauthored works were written by mixed-gender teams.

Table 2. Research Method by Type of Article for Papers Published in 10 Sociology Journals in the Early (1974-78) and Late (1979-83) Periods

	& Quantitative		% Qualitative		% Other Type		Total N	
	Early	Late	Early	Late	Early	Late	Early	Late
<i>Article Type*</i>								
Female gender	76.8	81.5	14.7	14.3	8.5	4.2	95	119
Male gender	86.9	94.8	1.7	6.1	3.5	7.1	99	115
Female other	62.7	66.4	20.6	14.2	16.8	19.4	101	113
Male other	71.0	72.9	10.3	10.3	18.7	16.8	107	107
Total (N)							402	454

\* For article type descriptions, see text and Table 1.

trend is in the same direction, but the magnitude of increase is even smaller. The positive association between gender topic and quantitative methods, regardless of author gender, appears in both periods.

Articles in the "other" category diminished from the early to the late period. Declines were modest for nongender articles, but steeper for gender articles. As articles focused on gender became more numerous in published sociological literature, theoretical articles and critiques became less common, although there was little change in the proportion of articles of this type in other subareas of sociology.

To examine the combined effects of author gender, article topic, and time on methods choice, we performed logistic regressions. We first estimated a model that also included as a predictor journal type (national mainstream—*AJS*, *ASR*; national specialty—*JHSB*, *SP*, *SPQ*, *SOE*, *WO*; and regional—*PSR*, *SF*, *SQ*), but this variable had no significant effect on methods use. Time period also had a nonsignificant effect on methods choice. We therefore report results in Table 3 of a logistic regression using author gender (1 = male, 0 = female), article topic (1 = nongender topic, 0 = gender topic), and a gender  $\times$  topic interaction term as predictors of methods choice (1 = quantitative, 0 = other).

The model accounts for .322 of the variance in methods choice. Author gender and article topic have significant effects on the probability that a published article used quantitative methods. Coefficients suggest that the magnitude of the effects of author gender and article topic is similar. Men were more likely than women to use quantitative methods. Gender articles more frequently than papers on other topics employed quantitative methods. There is a marginally significant ( $p < .10$ ) interaction between author gender and article topic. Writing on gender, rather than on other topics, resulted in a slightly increased probability that male-authored rather than female-authored articles would be quantitative.

Table 3 shows probabilities under the fitted model of using quantitative methods for each

topic-by-author category. There is a .20 greater probability that male-authored articles on gender were quantitative than male-authored articles on other topics. For women, writing on gender resulted in a .13 increase in probability that an article used quantitative methods.

## DISCUSSION

Our findings support the existence of systematic links between gender and methods, but they are not entirely consistent with the patterns we had anticipated. We find support for feminist scholars' claims that use of qualitative methods is significantly more common among women than men scholars who publish in major sociological journals. However, for both genders quantitative methods are the most common choice. A trend toward greater use of quantitative methods over time during the decade proved to be nonsignificant, perhaps because quantitative approaches had become the modal form for

Table 3. Logistic Regression Predicting Method Type\* (Quantitative or Other) by Author Gender and Article Topic (Nongender, Gender) for Published Articles in 10 Sociology Journals, 1984-83

	Regression Coefficient	Standard Error	Probability
Intercept	1.352	.169	.0000
Author gender <sup>b</sup>	.977	.294	.0009
Article Topic <sup>c</sup>	-.836	.220	.0001
Author Gender $\times$ Article Topic	-.596	.359	.0971
Model $\chi^2 = 52.2$ , d.f. = 3      Explained variance = .322			
Predicted probability under fitted model of using quantitative methods for four categories of articles:			
Female author, gender article			.794
Male author, gender article			.911
Female author, nongender article			.626
Male author, nongender article			.710

\* Coded 1 = Quantitative, 0 = Other.

<sup>b</sup> Coded 1 = Male, 0 = Female, based on gender of first author.

<sup>c</sup> Coded 1 = Nongender topic, 0 = Gender topic.

publications in these sources before 1974, the earliest year we sampled.

The associations between methods and topic of article were opposite those anticipated by feminist scholars and theorists, however. Writing about gender increased the probability that women's and men's work would be quantitative. There are two explanations for this finding, neither of which can be tested by our data. First, feminist critics might have been incorrect in their assertions that quantitative methods were less suited than qualitative methods for studying women's lives and gender issues. Their expectations for such an association might also be outdated. More sophisticated quantitative training for recently graduated sociologists, movement of women into "cash-nexus" domains of society, and greater availability of data suitable for quantitative analysis on women and gender might have reversed an earlier association between methods and topic.

A contrasting interpretation is that research about gender, a relatively new and perhaps not fully legitimated topic of inquiry among all sociologists (see Simeone 1987), might have been more palatable to editors and reviewers of mainstream journals if it used methods dominant within the discipline. Qualitative papers on gender might have represented double nonconformity, reducing the likelihood of acceptance for publication in the journals we reviewed. It is also possible that their authors might have avoided submission to mainstream journals, selecting instead one of the three sociology journals specializing in qualitative methods (*Qualitative Sociology*, *Symbolic Interaction*, or *Urban Life*, recently retitled *Journal of Contemporary Ethnography*).<sup>3</sup> Had we studied these journals, or other publication outlets such as books or interdisciplinary journals, associations

might have been different. This topic merits further investigation.

The relative scarcity of theoretical papers, integrative literature reviews, and critiques focused on gender in comparison to other topics is indirectly supportive of the second interpretation. These types of papers, which sometimes are commissioned or solicited by journal editors and which often are lead articles, mark certain topics to a general sociological readership as critical and central within a discipline. Integrative reviews and theoretical pieces on gender have been published, but more frequently in interdisciplinary journals or reviews put out by commercial publishers. Their absence from mainstream sociological journals raises the possibility that those who control journal content have not seen gender issues as central to contemporary sociology.

Gatekeepers might channel away from major journals research that represents nonconformity in both methods and topic. It also is possible that researchers might choose to publish these works in other outlets. Regardless of the reasons for the scarcity of qualitative gender articles in mainstream journals, the omission risks ghettoizing such research. The work might be sought out by those with strong interests in gender topics and/or nonquantitative methods, but qualitative gender articles are less likely to be seen by a broad spectrum of sociologists and hence less likely to influence development of the discipline. The process might lead to what Mackie (1985) has termed "methodological cooptation" of scholars interested in gender, who learn that the most certain route to publication in mainstream journals is quantification.

Our findings also suggest that male sociologists, whether by choice or constraint, have played a distinctive, rather narrow role in journal publishing on gender. As solo or senior coauthors, they have nearly always published quantitative papers, usually based on multivariate analysis of large datasets. They have participated very little in qualitative or interpretive studies of women's or of men's lives.

Our lack of data on submissions and evaluations of manuscripts does not allow us to test these alternative hypotheses, but the topic is worthy of future research. It also will be important in future research to discover whether women scholars more so than men (or scholars writing on gender more so than those writing on other topics) are bimethodological, using mainstream methods when publishing in major sociological journals but qualitative and nondatabase approaches when publishing elsewhere. Mackie's (1985) study suggests this is the case, at least for women scholars, but is flawed because it is unclear whether the same or

<sup>3</sup> As one anonymous reviewer for ASR pointed out, the pool of articles for major journals is influenced by the availability of alternative outlets for publication. The three qualitatively oriented journals undoubtedly competed with major journals for publishable qualitative papers (though all three were not published in some years included in our study). It is not clear, however, whether qualitative journals were more or less apt than major sociological journals to attract or publish gender articles. A second anonymous reviewer suggested that qualitative gender articles appearing in major journals in the years we studied, though scarce, were of exceptionally high quality, and perhaps, therefore, quite influential on the discipline. Consistent with this reviewer's observation, ASR editor William Form noted to us it was his impression that qualitative rather than quantitative articles were more frequently rejected by their reviewers, who usually are qualitative researchers themselves. These three scholars' observations suggest less consensus and more stringent criteria for evaluating qualitative articles, though not necessarily those focused on gender.

different sociologists published in the two sources. It also would be useful to explore in depth, using qualitative approaches such as intensive interviewing, what underlies choices of method in sociological research and the extent to which the choice is related to author gender, topic, training, the influence of mentors, the nature of research questions, the availability of data, and the perceived preferences of reviewers and editors of the desired outlets for publication.

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# REVIEWERS FOR THE 1987 ISSUE

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Cohen, Larry  
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Cornell, Stephen  
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Hamilton, V. Lee  
Hammond, Philip  
Hannan, Michael  
Hansmann, Henry  
Hargens, Lowell L.  
Hawke, Jo  
Hawkins, Richard  
Hawley, Amos H.  
Haydu, Jeffrey  
Hayward, Mark  
Hazelrigg, Lawrence  
Heiss, Jerold  
Heritage, John  
Heydebrand, Wolf  
Hibbs, Douglas  
Hicks, Alex  
Himmelfarb, Harold  
Hinkle, Gisela  
Hinkle, Roscoe  
Hirschi, Travis W.  
Hoge, Dean  
Hollander, Paul

- Hope, Keith  
 Horwitz, Allan  
 Horowitz, Ruth  
 Houghland, James G.  
 Houseknecht, Sharon  
 Howard, Judy  
 Huber, Bettina  
 Huber, Joan  
 Huff, Toby  
 Hunt, Janet  
 Inkeles, Alex  
 Isaac, Larry  
 Jackman, Robert W.  
 Jackson, Pamela Irving  
 Jacobs, David  
 Jacobs, Jerry  
 Jacobs, Norman  
 James David  
 Jasso, Guillermina  
 Jenkins, Craig  
 Kahn, Joan  
 Kalberg, Stephen  
 Kalleberg, Arne  
 Kantner, John  
 Kasarda, John D.  
 Kaufman, Robert  
 Kelly, John R.  
 Kemper, Theodore  
 Kenney Martin  
 Kerbo, Harold  
 Kerckhoff, Alan C.  
 Kessler, Ronald  
 Kick, Ed  
 Killingsworth, Mark  
 Klandermans, Bert  
 Kleck, Gary  
 Kluegel, James  
 Knoke, David  
 Koenig, Tom  
 Kohn, Melvin  
 Krause, Elliott  
 Krivo, Lauren  
 Krohn, Marvin  
 Krymkowski, Daniel  
 Ladinsky, Jack  
 Lamanna, Mary Anne  
 Lamont, Michele  
 Land, Kenneth  
 Lankford, John  
 Lee, Barrett  
 Leggett, John  
 Lehman, Edward W.  
 Leicht, Kevin  
 Leiter, Jeffrey  
 Lenski, Gerhard  
 Leslie, Gerald  
 Lewis, Jerry  
 Lewis, Michael  
 Leys, Colin  
 Li, Wen  
 Lieberman, Stanley  
 Light, Ivan H.  
 Lincoln, James  
 Linz, Juan  
 Lipset, Seymour Martin  
 Lizotte, Alan  
 Loftin, Colin K.  
 Loftland, John  
 Lopreato, Joseph  
 Lord, George, III  
 Loscocco, Karyn  
 Lukes, Steven  
 Lundman, Richard  
 McAdam, Douglas  
 McCall, George  
 McCarthy, James  
 McCarthy, John  
 McCartney, James L.  
 McDowall, David  
 McLanahan, Sara  
 McNamera, Patrick  
 Machalek, Richard  
 Marini, Margaret  
 Maris, Ronald  
 Marsden, Peter  
 Marsh, Robert  
 Marwell, Gerald  
 Massey, Douglas S.  
 Matras, Judah  
 Matsueda, Ross  
 Mauss, Armand  
 Maxim, Paul  
 Mayhew, Bruce  
 Mechanic, David  
 Meisner, Martin  
 Meltzer, Bernard  
 Menaghan, Betty  
 Messinger, Sheldon  
 Messner, Steven  
 Meyer, John  
 Meyer, Katherine  
 Meyer, Marshall W.  
 Mieth, Terance  
 Miller, Art  
 Miller, Joanne  
 Miller, Jon P.  
 Minitz, Beth  
 Mitchell, Robert  
 Mizruchi, Mark  
 Moch, Michael  
 Molm, Linda D.  
 Molotch, Harvey L.  
 Montero, Darrel M.  
 Moore, Kristin A.  
 Morgan, Philip  
 Morris, Aldon  
 Morrison, Denton  
 Mortimer, Jeylan  
 Mott, Frank  
 Mueller, Charles W.  
 Mukerji, Chandra  
 Mullins, Nicholas C.  
 Mutchler, Jan  
 Myers, Donell  
 Myles, John  
 Nagel, Joane  
 Nagi, Saad  
 Namboodiri, N.K.  
 Nelson, Hart  
 Nemeth, Charlan  
 Niedert, Lisa  
 Nolan, Patrick  
 Nonet, Philippe  
 Oakes, Guy  
 Ofshe, Richard J.  
 Olneck, Michael R.  
 Olzak, Susan  
 Orloff, Ann  
 Palmer, Donald  
 Pampel, Fred  
 Parker, Robert  
 Parcel, Toby L.  
 Paternoster, Raymond  
 Pearlin, Leonard  
 Pedraza-Bailey, Silvia  
 Perrow, Charles B.  
 Perrucci, Robert  
 Persell, Caroline  
 Pescosolido, Bernice  
 Peterson, Richard  
 Peterson, Richard R.  
 Peterson, Ruth  
 Petras, James  
 Pfeffer, Jeffrey  
 Phillips, David  
 Pinard, Maurice  
 Pincus, Fred L.  
 Portes, Alejandro  
 Powell, Michael  
 Powell, Walter W.  
 Price, James  
 Pullum, Thomas  
 Putnam, George  
 Quadagno, Jill  
 Quinney, Richard  
 Ragin, Charles  
 Reif, Linda  
 Reinharz, Shulamit  
 Reitzes, Dietrich  
 Reskin, Barbara  
 Rexroat, Cynthia  
 Reynolds, Paul  
 Richardson, Laurel  
 Riley, Matilda  
 Rindfuss, Ronald  
 Risman, Barbara  
 Robinson, Robert  
 Roby, Pamela  
 Roof, W. Clark  
 Roos, Patricia  
 Rosenberg, Morris  
 Rosenfeld, Rachel  
 Rosenfeld, Richard  
 Ross, Catherine E.  
 Roth, Guenther  
 Rothschild, Joyce  
 Roy, William G.  
 Rozman, Gilbert  
 Rubin, Beth  
 Robinson, Richard  
 Rudel, Thomas  
 Rueschemeyer, Dietrick  
 Rule, John  
 Sabel, Charles  
 Salehi, Mohammed M.  
 Sandefur, Gary  
 Sanders, Jimmy  
 Sarfatti-Larson, Magali  
 Schegloff, Emmanuel  
 Schoenberg, Sandra  
 Schoenherr, Richard A.



## REVIEWERS FOR THE 1987 ISSUE

Schooler, Carmi  
 Schur, Edwin  
 Schurmann, Franz  
 Schwartz, Mildred A.  
 Schwartz, Pepper  
 Schwirian, Kent P.  
 Sciulli, David  
 Scott, Joseph E.  
 Scott, W. Richard  
 Scully, Diana  
 Seeman, Melvin  
 Senter, Mary  
 Serpe, Richard  
 Sewell, William H., Jr.  
 Shalin, Dmitri  
 Short, James F.  
 Shrum, Wesley  
 Sica, Alan  
 Sinkus, Albert  
 Simon, Rita James  
 Skocpol, Theda  
 Skvortez, John  
 Smith-Lovin, Lynn  
 Snipp, Matthew  
 Snow, David  
 Sobel, Michael  
 Sorensen, Aage  
 Sorensen, Annemette  
 South, Scott  
 Southwood, Kenneth  
 Spaeth, Joe  
 Spenner, Kenneth  
 Spilerman, Seymour  
 Spitze, Glenna  
 Stack, Steven  
 Stahura, John  
 Stark, Rodney W.  
 Statham, Anne  
 Stearns, Linda  
 Stewman, Shelby  
 Stinchcombe, Arthur

Stipp, Horst  
 St. John, Craig  
 Stokes, Randall G.  
 Strodbeck, Fred  
 Suchindran, C.M.  
 Summers, Gene  
 Suttles, Gerald  
 Swanson, Guy  
 Swidler, Ann  
 Sykes, Richard  
 Szelenyi, Ivan  
 Tanur, Judith  
 Teachman, Jay  
 Thomson, Elizabeth  
 Thornberry, Terence  
 Thorne, Barrie  
 Tickamyer, Ann  
 Tienda, Marta  
 Tilly, Charles H.  
 Tilly, Louise  
 Timberlake, Michael  
 Tipton, Steven  
 Tiryakian, Edward  
 Title, Charles R.  
 Tolbert, Charles  
 Treas, Judith  
 Turk, Austin T.  
 Turner, Joannathan  
 Tyrce, Andrea  
 Udry, Richard  
 Useem, Bert  
 Useem, Michael  
 Vallas, Steven  
 Van den Berg, Axel  
 Van den Berghe, Pierre  
 Vanneman, Reeve  
 Villemez, Wayne J.  
 Vogel, Ezra  
 Wagner, David  
 Waite, Linda J.  
 Walby, Sylvia

Walder, Andrew  
 Wallace, Michael  
 Walsh, Edward  
 Walters, Pamela  
 Walton, John  
 Ward, Kathryn B.  
 Wardell, Mark  
 Warr, Mark  
 Warriner, Keith  
 Wasserman, Ira  
 Wasserman, Stanley  
 Weisburd, David  
 Wellman, Barry  
 West, Candace  
 Whyte, Martin King  
 Whyte, William F.  
 Wiley, Norbert  
 Williams, J. Douglas  
 Williamson, Oliver E.  
 Wilson, Franklin  
 Wilson, John  
 Wilson, Thomas  
 Wilson, Thomas P.  
 Wiley, Mary Glenn  
 Wipper, Audrey  
 Wohlstein, Ron  
 Wolff, Kurt  
 Wright, Erik O.  
 Wright, James  
 Wrong, Dennis H.  
 Wu, Lawrence  
 Wuthnow, Robert  
 Yancey, William L.  
 Yinger, J. Milton  
 Zablocki, Benjamin  
 Zald, Mayer N.  
 Zaret, David  
 Zatz, Marjorie  
 Zelditch, Morris Jr.

# INDEX

## AMERICAN SOCIOLOGICAL REVIEW

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### CONTENTS OF VOLUME 52, NUMBERS 1-6

#### ARTICLES

<b>Abrahamson, Mark and Lee Sigelman</b> Occupational Sex Segregation in Metropolitan Areas.....	588
<b>Alexander, Karl L., Doris R. Entwisle, and Maxine S. Thompson</b> School Performance, Status Relations, and the Structure of Sentiment: Bringing the Teacher Back In.....	665
<b>Allison, Paul D.</b> Interuniversity Mobility of Academic Scientists.....	643
<b>Alwin, Duane F.</b> Distributive Justice and Satisfaction with Material Well-Being.....	83
<b>Bakanic, Von, Clark McPhall, and Rita J. Simon</b> The Manuscript Review and Decision-Making Process.....	631
<b>Baron, James N.</b> See Wharton, Amy S.....	574
<b>Billy, John O. G.</b> See Udry, J. Richard.....	841
<b>Bongaarts, John</b> See Watkins, Susan Cotts.....	346
<b>Bradshaw, York W.</b> Urbanization and Underdevelopment: A Global Study of Modernization, Urban Bias, and Economic Dependency.....	224
<b>Brown, Courtney</b> Voter Mobilization and Party Competition in a Volatile Electorate..	59
<b>Burris, Val</b> The Political Partisanship of American Business: A Study of Corporate Political Action.....	732
<b>Burton, Michael G. and John Higley</b> Elite Settlements.....	295
<b>Camic, Charles</b> The Making of a Method: A Historical Reinterpretation of the Early Persons.....	421
<b>Cheek, Nell H., Jr.</b> See Shrum, Wesley.....	218
<b>Cohen, Lawrence E. and Kenneth C. Land</b> Age Structure and Crime: Symmetry Versus Asymmetry and the Projection of Crime Rates through the 1990s.....	170
<b>Denton, Nancy A.</b> See Massey, Douglas S.....	802
<b>de Vaus, David and Ian McAllister</b> Gender Differences in Religion.....	472
<b>DiMaggio, Paul</b> Classification in Art.....	440
<b>Entwisle, Doris R.</b> See Alexander, Karl L.....	665
<b>Frey, William H.</b> Migration and Depopulation of the Metropolis: Regional Restructuring or Rural Renaissance?.....	240
<b>Filgstein, Neil</b> The Intraorganizational Power Struggle: Rise of Finance Personnel to Top Leadership in Large Corporations, 1919-1979.....	44
<b>Furstenberg, Frank F., Jr., S. Philip Morgan, Kristen A. Moore, and James L. Peterson</b> Race Differences in the Timing of Adolescent Intercourse.....	511
<b>Granfors, Mark W.</b> See Halliday, Terence C.....	456
<b>Greeley, Andres M.</b> See Hout, Michael.....	325
<b>Halliday, Terence C., Michael J. Powell, and Mark W. Granfors</b> Minimalist Organizations: Vital Events in State Bar Associations, 1870-1930.....	456
<b>Hallinan, Maureen T. and Richard A. Williams</b> The Stability of Students' Interracial Friendships.....	653
<b>Heimer, Karen</b> See Matsueda, Ross L.....	826
<b>Higley, John</b> See Burton, Michael G.....	295
<b>Hout, Michael and Andrew M. Greeley</b> The Center Doesn't Hold: Church Attendance in the United States, 1940-1984.....	325
<b>Kahn, Joan R. and William M. Mason</b> Political Alienation, Cohort Size, and the Easterlin Hypothesis.....	155
<b>Klandermans, Bert and Dirk Oegema</b> Potentials, Networks, Motivations, and Barriers: Steps Toward Participation in Social Movements.....	519
<b>Kohn, Melvin L.</b> Cross-National Research as an Analytic Strategy.....	713
<b>Knottnerus, J. Davis</b> Status Attainment Research and Its Image of Society.....	113
<b>Krauze, Tadeusz K.</b> See Slomczynski, Kazimeriz Z.....	598
<b>Land, Kenneth C.</b> See Cohen, Lawrence E.....	170

- Link, Bruce G.** Understanding Labeling Effects in the Area of Mental Disorders: An Assessment of the Effects of Expectations of Rejection ..... 96
- London, Bruce** Structural Determinants of Third World Urban Change: An Ecological and Political Economic Analysis ..... 28
- Long, J. Scott** See Allison, Paul D. .... 643
- Long, J. Scott** See Miethe, Terance D. .... 184
- Mason, William M.** See Kahn, Joan R. .... 155
- Massey, Douglas S. and Nancy A. Denton** Trends in the Residential Segregation of Blacks, Hispanics, and Asians: 1970-1980 ..... 802
- Matsueda, Ross L. and Karen Heimer** Race, Family Structure, and Delinquency: A Test of Differential Association and Social Control Theories ..... 826
- McAllister, Ian** See de Vaus, David ..... 472
- McPhail, Clark** See Bakanic, Von ..... 631
- McPherson, J. Miller and Lynn Smith-Lovin** Homophily in Voluntary Organizations: Status Distance and the Composition of Race-to-Race Groups ..... 370
- Menken, Jane A.** See Watkins, Susan Cotts ..... 346
- Miethe, Terance D., Mark C. Stafford, and J. Scott Long** Social Differentiation in Criminal Victimization: A Test of Routine Activities/Lifestyle Theories ..... 184
- Moore, Kristin A.** See Furstenberg, Frank F., Jr. .... 511
- Morgan, S. Phillip** See Furstenberg, Frank F., Jr. .... 511
- Nee, Victor** See Sanders, Jimmy M. .... 745
- Oegema, Dirk** See Klandermans, Bert ..... 519
- Ortiz, Vilma** See Tienda, Marta ..... 195
- Peterson, James L.** See Furstenberg, Frank F., Jr. .... 511
- Powell, Michael J.** See Halliday, Terence C. .... 456
- Rau, William and Dennis W. Roncek** Industrialization and World Inequality: The Transformation of the Division of Labor in 59 Nations, 1960-1981 ..... 359
- Ridgeway, Cecilia L.** Nonverbal Behavior, Dominance, and the Basis of Status in Task Groups ..... 683
- Riley, Matilda White** On the Significance of Age in Sociology ..... 1
- Rindfuss, Ronald R., C. Gray Swicegood, and Rachel A. Rosenfeld** Disorder in the Life Course: How Common and Does it Matter? ..... 785
- Roncek, Dennis W.** See Rau, William ..... 359
- Rosenfeld, Rachel A.** See Rindfuss, Ronald R. .... 785
- Ross, Christopher O.** Organizational Dimensions of Metropolitan Dominance: Prominence in the Network of Corporate Control, 1955-1975 ..... 258
- Sanders, Jimmy M. and Victor Nee** Limits of Solidarity in the Enclave Economy ..... 771
- Shepelak, Norma J.** The Role of Self-Explanations and Self-Evaluations in Legitimizing Inequality ..... 495
- Shrum, Wesley and Neil H. Cheek, Jr.** Social Structure During the School Years: Onset of the DeGrouping Process ..... 218
- Sigelman, Lee** See Abrahamson, Mark ..... 588
- Simon, Rita J.** See Bakanic, Von ..... 631
- Slomczynski, Kazimierz Z. and Tadeusz K. Krause** Cross-Nationality Similarity in Social Mobility Patterns: A Direct Test of the Featherman-Jones-Hauser Hypothesis ..... 598
- Smith, Shelley A.** See Tienda, Marta ..... 195
- Smith-Lovin, Lynn** See McPherson, J. Miller ..... 370
- Stafford, Mark C.** See Miethe, Terance D. .... 184
- Stahura, John M.** Suburban Socioeconomic Status Change: A Comparison of Models, 1950-1980 ..... 268
- Stevens, Gillian and Gray Swicegood** The Linguistic Context of Ethnic Endogamy .. 73
- Stolte, John F.** The Formation of Justice Norms ..... 774
- Sutton, John R.** Doing Time: Dynamics of Imprisonment in the Reformist State ..... 612
- Swicegood, Gray** See Stevens, Gillian ..... 73
- Swicegood, C. Gray** See Rindfuss, Ronald R. .... 785
- Szelényi, Szonja** Social Inequality and Party Membership: Patterns of Recruitment into the Hungarian Socialist Workers' Party ..... 559
- Taylor, Patricia A.** The Celebration of Heroes Under Communism: On Honors and the Reproduction of Inequality ..... 143
- Thompson, Maxine S.** See Alexander, Karl L. .... 665

<b>Tienda, Marta, Shelley A. Smith, and Vilma Ortiz</b> Industrial Restructuring, Gender Segregation, and Sex Differences in Earnings .....	195
<b>Tolnay, Stewart E.</b> The Decline of Black Marital Fertility in the Rural South: 1910–1940 .....	211
<b>Tuch, Steven A.</b> Urbanism, Region, and Tolerance Revisited: The Case of Racial Prejudice .....	504
<b>Turner, Jonathan H.</b> Toward a Sociological Theory of Motivation .....	15
<b>Udry, J. Richard and John O.G. Billy</b> Initiation of Coitus in Early Adolescence .....	841
<b>Watkins, Susan Cotts, Jane A. Menken, and John Bongaarts</b> Demographic Foundations of Family Change .....	346
<b>Wharton, Amy S. and James N. Baron</b> So Happy Together? The Impact of Gender Segregation on Men at Work .....	574
<b>Williams, Richard A.</b> See Hallinan, Maureen T. ....	653
<b>Well, Frederick D.</b> Cohorts, Regimes, and the Legitimization of Democracy: West Germany Since 1945 .....	308
<b>Yamaguchi, Kazuo</b> Models for Comprising Mobility Tables: Toward Parsimony and Substance .....	482

## RESEARCH NOTES

<b>Allison, Paul D.</b> See Furstenberg, Frank F., Jr. ....	695
<b>Dietz, Thomas R., Scott Frey, and Linda Kalof</b> Estimation with Cross-National Data: Robust and Nonparametric Methods .....	380
<b>Drass, Kriss A.</b> See Kiser, Edgar. ....	286
<b>Frey, R. Scott</b> See Dietz, Thomas .....	380
<b>Furstenberg, Frank F., Jr., S. Philip Morgan, and Paul D. Allison</b> Paternal Participation and Children's Well-Being After Marital Dissolution .....	695
<b>Goldscheider, Calvin and Francis K.</b> Moving Out and Marriage: What Do Young Adults Expect? .....	278
<b>Goldscheider, Francis K.</b> See Goldscheider, Calvin .....	278
<b>Grant, Linda, Kathryn B. Ward and Xue Lan Rong</b> Is There an Association between Gender and Methods in Sociological Research? .....	856
<b>Harer, Miles D.</b> See Steffensmeier, Darrell .....	702
<b>Kalof, Linda</b> See Dietz, Thomas .....	380
<b>Kingston, Paul William and Steven L. Nock</b> Time Together Among Dual-Earner Couples .....	391
<b>Kiser, Edgar and Kriss A. Drass</b> Changes in the Core of the World-System and the Production of Utopian Literature in Great Britain and the United States, 1883–1975 .....	286
<b>Marsden, Peter V.</b> Core Discussion Networks of Americans .....	122
<b>Morgan, S. Philip and Linda J. Waite</b> Parenthood and the Attitudes of Young Adults .....	541
<b>Morgan, S. Philip</b> See Furstenberg, Frank F., Jr. ....	695
<b>Nock, Steven L.</b> See Kingston, Paul William .....	391
<b>Rong, Xue Lan</b> See Grant, Linda. ....	856
<b>Stack, Steven</b> Celebrities and Suicide: A Taxonomy and Analysis, 1948–1983 .....	401
<b>Stack, Steven</b> Publicized Executions and Homicide, 1950–1980 .....	532
<b>Steffensmeier, Darrell, Cathy Streifel, and Miles D. Harer</b> Relative Cohort Size and Youth Crime in the United States, 1953–1984 .....	702
<b>Streifel, Cathy</b> See Steffensmeier, Darrell .....	702
<b>Teachman, Jay D.</b> Family Background, Educational Resources, and Educational Attainment .....	548
<b>Waite, Linda J.</b> See Morgan, S. Philip .....	541
<b>Ward, Kathryn B.</b> See Grant, Linda .....	856

## COMMENTS

<b>Bradshaw, York and Larry Radbill</b> Method and Substance in the Use of Ratio Variables (Comment on Firebaugh and Gibbs, <i>ASR</i> , October 1985) .....	132
<b>Breiger, Ronald L. and Jerry A. Jacobs</b> On Occupational Mobility and Social Class (Comment on Snipp, <i>ASR</i> , August 1985) .....	413

<b>Firebaugh, Glenn and Jack P. Gibbs</b> Defensible and Indefensible Commentaries (Reply to Kraft and Bradshaw/Radbill, <i>ASR</i> , February 1987) .....	136
<b>Gibbs, Jack P.</b> See Firebaugh, Glenn .....	136
<b>Hauser, Robert M.</b> Sharing Data: It's Time for ASA Journals to Follow the Folkways of a Scientific Sociology (Guest Editorial, <i>ASR</i> , December 1987).....	vi
<b>Jacobs, Jerry A.</b> See Breiger, Ronald L. ....	413
<b>Jensen, Leif</b> See Portes, Alejandro.....	768
<b>Kraft, Manfred</b> On "User's Guide to Ratio Variables" (Comment on Firebaugh and Gibbs, <i>ASR</i> , October 1985).....	135
<b>Nee, Victor and Jimmy Sanders</b> On Testing the Enclave-Economy Hypothesis (Reply to Portes, <i>ASR</i> , December 1987).....	771
<b>Portes, Alejandro and Leif Jensen</b> What's an Ethnic Enclave? The Case for Conceptual Clarity (Comment on Sanders, <i>ASR</i> , December 1987) .....	768
<b>Radbill, Larry</b> See Bradshaw, York .....	132
<b>Sanders, Jimmy</b> See Nee, Victor .....	771
<b>Snipp, C. Matthew</b> More on Occupational Mobility and Social Class (Reply to Breiger and Jacobs, <i>ASR</i> , June 1987) .....	416

# ASA JOURNALS

## Current and Forthcoming Articles

### Social Psychology Quarterly

Vol. 50, No. 4 (December 1987)

- Can Functions Be Measured? A New Perspective on the Functional Approach to Attitudes, *Gregory M. Herek*
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- Norms, Preferences, Identities, and Retention Decisions, *Bruce J. Biddle, Barbara J. Bank, and Ricky L. Slavings*
- Information Patterns, Attribution, and Attraction, *Charles E. Kimble and Frank R. Kardes*
- A Confirmatory Factor-Analytic Model of Alienation, *Bruce R. Roberts*
- Corrigendum: Methodology, Technology, and Serendipity, *Robert J. Johnson and Howard B. Kaplan*

### Sociology of Education

Vol. 60, No. 4 (October 1987)

- School Desegregation: An Exchange, *Finis Welch*
- Reply to Welch, *Franklin Wilson*
- Differences Among School Outputs and Educational Production Functions, *Anastasia Kostakis*
- College Attendance and Attitude Change: A Panel Study 1970-81, *Fern K. Willits and Richard B. Funk*
- Unionization, Institutional Orientation and the Economic Worth of the Faculty, *Pamela Irving Jackson and Roger Clark*
- Children Who Do Exceptionally Well in First Grade, *Aaron Pallas, Doris Entwisle, Karl Alexander, and Doris Cadigan*

### Sociology of Education

Vol. 61, No. 1 (January 1988)

- Routines in the Peer Culture of American and Italian Nursery School Children, *William A. Corsaro*


- Women's Labor Force Participation and Preschool Enrollment: A Cross-National Perspective, 1965-1980, *Sorca M. O'Connor*

- Returning to School and Women's Occupational Attainment, *Diane H. Felmlee*
- Schooling, Tracking, and Teenage Intelligence, *Yossi Shavit and David L. Featherman*
- Educational Credentials and Promotions: Does Schooling Do More than Get You in the Door? *David Bills*

### Contemporary Sociology

Vol. 17, No. 1 (January 1988)

- Patricia Caplan, *Class and Gender in India: Women and Their Organizations in a South Indian City*. Lina Fruzzetti and Marilyn Rueschemeyer
- Susan Dorsky, *Women of Amran: A Middle Eastern Ethnographic Study*. Lina Fruzzetti and Marilyn Rueschemeyer
- Scarlett T. Epstein, Kate Crehan, Annemarie Gerzer, and Jürgen Sass, *Women, Work, and Family in Britain and Germany*. Lina Fruzzetti and Marilyn Rueschemeyer
- Joanna Liddle and Rama Joshi, *Daughters of Independence: Gender, Caste, and Class in India*. Lina Fruzzetti and Marilyn Rueschemeyer
- Elizabeth Vallance and Elizabeth Davies, *Women of Europe: Women MEPs and Equality Policy*. Lina Fruzzetti and Marilyn Rueschemeyer
- Michael Goldfield, *The Decline of Organized Labor in the United States*. William Form
- John A. Hall, *Powers and Liberties: The Causes and Consequences of the Rise of the West*. Jack A. Goldstone
- Evelyn Huber Stephens and John D. Stephens, *Democratic Socialism in Jamaica: The Political Movement and Social Transformation in Dependent Capitalism*. Walter Korpi
- J.A. Mangan, *The Games Ethnic and Imperialism: Aspects of the Diffusion of an Ideal*. Gary Alan Fine
- Michael Rosenthal, *The Character Factory: Baden Powell's Boy Scouts and the Imperatives of Empire*. Gary Alan Fine

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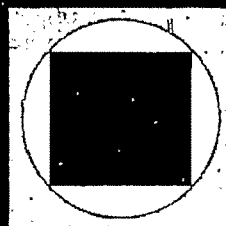
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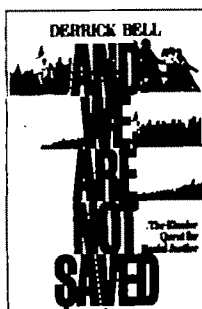
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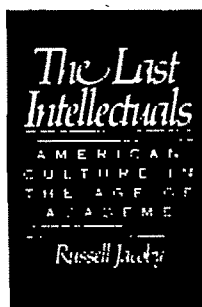
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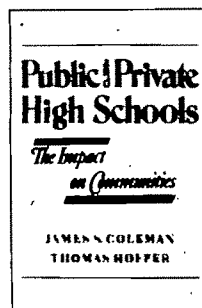
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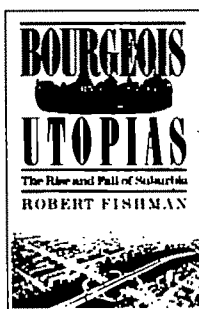
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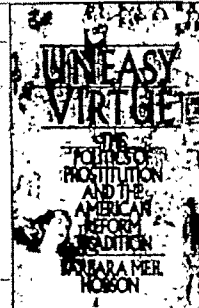
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